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CONTENTS

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THE NEW PHYSIOLOGY[1]

LOOKING back on the history of physiology we can see that there have been various turning-points in general physiological theory, and consequently in the trend of research. Particular discoveries or series of discoveries, often in allied sciences, have led to these turning-points.

The last great turning-point in physiology was about the middle of last century. Up till then it was generally held that in a living organism a specific influence, the so-called "vital force," controls the more intimate and important physiological processes. Inspired by the rapid advances of physics and chemistry, the younger physiologists of that time broke away from vitalism, and maintained that all physiological change is subject to the same physical and chemical laws as in the inorganic world, so that in ultimate analysis biology is only a branch of physics and chemistry.

The subsequent progress of physiology has shown that all, without exception, of the physical and chemical hypotheses then advanced in explanation of intimate physiological processes were far too simple to explain the facts; but the general conclusion that biology is only a special application of ordinary physics and chemistry became firmly established, and is still what may be called the orthodox creed of physiologists. It may be truly said that most physiologists look upon this creed as something which has been established for all time, and that they would be inclined to regard any deviation from it as harmful

[1] A lecture delivered before the Harvey Society, New York, October 14, 1916.

scientific heresy. Nevertheless I think that we have again reached a turning-point, and that a new physiology is arising in place of the physico-chemical physiology which has held sway for so many years. I propose in this lecture to give some account of how, as it seems to me, this new physiology is shaping itself.

It is natural for us to assume that the aim of all investigations in physiology must be to ascertain the causes of physiological activity. However complex a physiological reaction may be, the conditions which determine it can be investigated experimentally; and from long experience we can be quite certain that such experimental investigation will always lead to some result, however obscure. There is, and can be, no limit to experimental investigation of causes. When, however, we examine the results obtained by experimental physiology there emerges a point in which they differ greatly from the results ordinarily obtained in the investigation of inorganic phenomena: for it is characteristic of physiological reactions that they are dependent to an extreme degree on all sorts of environing conditions. We recognize this when we speak of stimulus and response rather than of cause and effect. When the light from a star is focused on the retina there is a physiological response by night, but none by day. The response evidently depends on the existing state of excitation of the whole retina. It also depends on the normal nutrition of the retina and brain. If the blood is abnormal in composition the ordinary response is interfered with; and we are as yet only at the beginnings of knowledge with regard to the minute changes in blood composition and other conditions of environment which are sufficient to affect the response very materially.

It is the same with every physiological response. The further we investigate the more evident does it become that each physiological response depends on a vast number of conditions in the environment of the responding tissue. On superficial investigation we do not realize this: for we can often get exactly the same response, time after time, with the same stimulus. To the attainment of this result it is only necessary to see that the conditions are "normal." It is only after more thorough investigation that we find that "normal conditions" imply something which is both extremely definite and endlessly complex. We then begin to realize that the maintenance of normal conditions is from the physical and chemical standpoint a phenomenon before which our wonder can never cease.

Physiological investigation of causes seems, thus, to lead us up to a tangled maze of causal conditions. He who looks for definite "causal chains" in physiological phenomena finds in place of them a network of apparently infinite complexity. The physiologists who led the revolt of last century against vitalism did not see this network. To them it seemed that there were probably simple physical and chemical explanations of the various physical and chemical changes associated with life. The progress of experimental physiology since that time has effectually shown that this was only a dream, and physiologists are now awakening from the dream.

But we are also awakening from another dream. About the middle of last century it seemed as if, in the current conceptions of matter and energy, we had reached finality as regards the inorganic world. The chemical atom, on the one hand, and the energy associated with it, on the other, seemed to represent bed-rock reality—a reality including not merely inorganic, but also organic phenomena. Discoveries con-

nected more particularly with electrical and electro-chemical phenomena, the periodic law, and radio-activity are awakening us from this dream also. The supposed bed-rock reality of a former generation seems to be melting down before our eyes. The solvent has been the study of particular phenomena, such as those of radio-activity. The professional physicists and chemists have hitherto kept away from the serious study of life. For the most part they have regarded life as something apart: or as a complex physical and chemical phenomenon which is not likely to throw any light on the deeper problems of physics and chemistry. In this attitude I think that they have been mistaken; but in any case it is evident that we must guard against the quite unwarranted assumption that the only possibility of advance in physiology is by the direct application to life of the physical and chemical ideas which held unchallenged sway for so many years.

In this reference I should like to reply to some remarks, made partly with reference to my own writings, by my friend Professor Macallum of Toronto, in a very able and interesting presidential address to the American Society of Biological Chemistry two years ago.[2] After frankly admitting that the apparent difficulties of the mechanistic interpretation of life "put a task upon the human spirit which is apparently not imposed thereon in the theoretic explanation of any other department of science" he proceeds to argue that this is because "our knowledge of the laws that operate in matter is as yet only a very remote approximation to the whole of the lore on this subject that is possibly attainable and that will be ultimately attained." He feels, however, that this defence of the mechanistic theory is somewhat dangerous, and therefore proceeds to point out "that though we know so little of the properties and laws of matter, we know it with a degree of certainty which is not exemplified in the case of any other department of the known or the knowable, and further that the most rational method of interpreting vital phenomena is to explain the unknown in terms of the known, to trace back the causation of the obscure and mysterious to the operations of wholly natural laws and processes."

Now with this latter sentiment I am in entire agreement; but I would point out that Professor Macallum had just invoked not what he considers the known, but, on the contrary, the totally unknown properties of matter, to furnish us with a future physico-chemical explanation of life. I confess that there is in his argument a certain theological smack which strongly appeals to me as a fellow Scotchman. In the domain of "Apologetics" he would, I feel sure, make a great impression. But in the domain of Natural Science we have to examine arguments somewhat closely, and it seems to me that his admissions, which are right and unavoidable, carry him so far that his defence of the mechanistic theory of life is wholly unconvincing. One can not get round the fact that the mechanistic theory has not been a success in the past, and shows no sign of being a success in the future.

When we look broadly at biological phenomena, it is evident that they are distinguished by one universal characteristic. The structure, activity and life history of an organism tend unmistakably to maintain a normal. Accident may destroy an organism, or even a whole species, but within limits of external environment which are the wider the more highly developed the organism is, the normal life history of each individual is fulfilled.

[2] *Journal of Biological Chemistry*, XVII., p. VIII., 1914.

If, now, we consider the advance of physiological knowledge from the standpoint of the efforts which have been made, not to ascertain the causes of vital activity, but to track out its normal details, the past history of physiology takes on a new aspect. It becomes a record, not of disheartening repulse before a hopeless wire entanglement, but of continuous progress. The new physiology of which I wish to speak to-night is a physiology which deliberately and consciously pursues this line of progress, leaving on one side what one may call the "causal" physiology handed down to us from the last generation. This new physiology is in one sense not new, but very old. It is only new in the sense of consciously pursuing an aim which has nearly always been instinctively pursued by physiologists, and particularly by the great physiologist from whom this society takes its name.

Now I think that many of my hearers will at once say that such a course may be useful up to a certain point, but that it is not true science, and that therefore we can not desert the old attempts. We must, in fact, still continue our frontal attacks on the wire entanglement. To this criticism I shall endeavor to reply later. But meanwhile I should like to explain more clearly, and by means of examples, what the new physiology aims at.

Perhaps I can do this most directly by referring first to the corner of physiology which has largely occupied my own attention—the physiology of breathing.

When we count the breaths, or measure their depth, we find much irregularity, as if there were no very definite or exact regulation of the breathing. Any active occupation, such as speaking or singing, interferes in various ways with the breathing, and the impression at first produced is that the regulation of breathing is very rough. It is also commonly believed that by special training we can increase, or "improve," the ventilation of the lungs. On the other hand it has been well known for long that the breathing is more or less regulated to correspond with the consumption of oxygen and production of carbon dioxide in the body. Thus during heavy muscular exertion greatly increased breathing accompanies the greatly increased oxidation in the tissues. Another fact, well known to physiologists, is that if the lung ventilation is by artificial or voluntary means greatly increased for a short time, there follows a period of "apnea," during which natural breathing is absent. The exact cause of this apnea was till recently obscure. In 1868 Hering and Breuer showed that the inflation of the lungs in inspiration gives rise to impulses passing up the vagus nerves, and inhibiting further inspiratory impulses from the respiratory center, at the same time starting expiration. Deflation of the lungs in expiration has a converse effect. So long as the vagi are intact they are constantly playing this game of battledore and shuttlecock with the respiratory center, and Hering called this the "self-regulation" (Selbst steuerung) of breathing. The apnea following excessive ventilation of the lungs was interpreted by subsequent physiologists as the summed inhibitory effect of repeated distentions. Fredericq showed, however, that apnea is produced when the respiratory center of one animal is supplied with blood from another animal the lungs of which are excessively ventilated. This, therefore, is a true "chemical" apnea, due to over-aeration of the arterial blood, and was distinguished from "vagus" apnea. Nevertheless the correlation of the various "factors" apparently involved in the regulation of breathing remained extremely obscure.

I observed that when the air breathed is gradually and increasingly vitiated by rebreathing it, or by what is known to miners as "black damp," the breathing is also increased, but not in any simple relation to the extent of the vitiation. With a steady increase in the vitiation the breathing at first increases only a little, but as the vitiation increases further the effect on the breathing is greater and greater. Thus an increase from 4 per cent. to 5 per cent. in the percentage of CO_2 in the inspired air produces about 20 times as great an effect on the breathing as an increase to 1 per cent. from the normal of 0.03 per cent. Observations of this kind suggested that the breathing is so regulated as to maintain a certain normal percentage of carbon dioxide in the air within the lungs, and that as the percentage in the inspired air rises a greater and greater increase in the breathing is required to maintain this normal. It is, moreover, excess of carbon dioxide that excites the breathing. A corresponding deficiency of oxygen has no such effect.

It was found by Mr. Priestley and myself that a sample of the air in contact with the blood in the lungs could easily be obtained by catching the latter part of the air expired in a deep inspiration. As we expected, the percentage of carbon dioxide in this air turned out to be on an average practically constant for each individual.

If the frequency of breathing is voluntarily varied, even as widely as from three a minute to 60 a minute, the depth adjusts itself so as to keep the average alveolar percentage of carbon dioxide almost absolutely steady; and conversely if the depth is varied. With resistance to breathing there is a similar effect. The effort put into inspiration and expiration is so increased as to overcome the resistance and keep the alveolar carbon dioxide almost steady. If the breathing is temporarily interrupted or abnormally increased, the time is made up afterwards, so that the average alveolar carbon dioxide percentage is practically steady. If, finally, the inspired air is vitiated by carbon dioxide, the breathing is so increased as to keep, if possible, the alveolar percentage approximately steady.

The effects discovered by Hering and Breuer appeared to them to depend simply on the state of mechanical distention of the lungs, and to have no relation to the chemical regulation of breathing. Mr. Mavrogordato and I have quite recently reinvestigated these phenomena in man. The results showed that the amounts of inflation or deflation needed to produce the Hering-Breuer effects depend entirely on the chemical stimulus of carbon dioxide. When this stimulus is absent, as in apnea, a very slight inflation or deflation will suffice, so that the breathing is, as it were, jammed during apnea; while if the chemical stimulus is strong it needs a great inflation or deflation to produce the Hering-Breuer effect. The vagi prevent useless prolongation of inspiratory or expiratory effort and consequent waste of time in breathing, or damage to the lung structure. They also coordinate the discharges of the center with actual inflations or deflations of the lungs. When the vagi are cut the breathing becomes slow, and, as Scott showed, can only imperfectly respond to an increased chemical stimulus, since the frequency can not be increased. The influence of the vagi is entirely in the direction of keeping the alveolar air normal. Perhaps nothing illustrates more clearly the dependence of nervous reactions on more fundamental physiological conditions than the varying response of the respiratory center to the stimulus of inflation or deflation of the lungs.

When excessive ventilation of the lungs

is so arranged that there is no fall in the alveolar percentage of carbon dioxide, no apnea follows. There is thus no such thing as the so-called vagus apnea. Apnea is simply due to excessive removal of carbon dioxide from the alveolar air.

When the barometric pressure is varied it becomes evident that the normal which dominates the regulation of breathing is not the percentage of carbon dioxide in the alveolar air, but the partial pressure or molecular concentration. At the normal atmospheric pressure of 30 inches there is about 5.6 per cent. of carbon dioxide in the alveolar air, but only 2.8 at 60 inches barometric pressure, and 1.4 at 120 inches. In these three cases the percentage of CO_2 varies widely, but the partial pressure is the same. It is only with constant barometric pressure that the normal percentage is steady.

When the breathing is increased by excess of CO_2 in the inspired air, or increased production of CO_2 in the body, there is, as might be expected, a slight rise in the alveolar CO_2 percentage. It is this slight rise that is the stimulus to increased breathing. Roughly speaking, a rise of 0.2 per cent. increases the resting breathing by 100 per cent., while a fall of 0.2 per cent. produces apnea. The stimulus of the increased CO_2 percentage is conveyed to the respiratory center by the blood. Under ordinary average conditions the center responds with normal breathing when the blood leaving the lungs is saturated with air containing 5.6 per cent. of CO_2, but does not respond at all when the blood is saturated with 5.4 per cent. of CO_2 or less. The threshold value of CO_2 is, however, greatly lowered by excessive administration of acids or in any condition of so-called acidosis, and is raised by alkalies or an alkaline diet. This and other evidence points to the fact that CO_2 acts on the respiratory center in virtue of its acid properties when in solution.

According to modern ideas the acidity or alkalinity of a liquid depends on its hydrogen ion concentration. The accurate measurement of the hydrogen ion concentration of blood by the electrometric method is attended with great difficulties; but these have been to a large extent overcome by Hasselbalch of Copenhagen, who has obtained measurements of the effects of saturation with different partial pressures of CO_2 on the hydrogen ion concentration of blood. He has also shown experimentally that when the alveolar CO_2 threshold is lowered or raised by an acid or alkaline diet this raising or lowering is just sufficient to keep the hydrogen ion concentration of the arterial blood sensibly steady. It is now certain, therefore, that what the respiratory center is reacting to when it reacts to a slight increase in the alveolar CO_2 percentage is the consequent slight increase in the hydrogen ion concentration of the blood.

The latter increase is so minute that it can only be detected electrometrically when it is of sufficient extent to produce very gross changes in the breathing. The respiratory center is enormously more delicate as an index of change in hydrogen ion concentration of the blood than any existing physical or chemical method.

As already remarked, the alveolar CO_2 percentage is extremely steady under ordinary resting conditions. This implies that the hydrogen ion concentration of the blood is regulated with almost incredible delicacy, and must be so regulated apart altogether from the breathing. The breathing simply regulates the rapid disturbances in hydrogen ion concentration caused by variations in the production of CO_2: other disturbances are regulated otherwise than by the breathing. There is clear evidence

that both the kidneys and the liver play a part in this regulation; but of the marvellous accuracy of the regulation physiologists had, till the recent work on the physiology of breathing, no clear conception.

It is not merely the hydrogen ion concentration of the blood that is accurately regulated, but also its capacity for taking up a constant amount of CO_2 in presence of a constant partial pressure of this gas. This capacity depends on the concentration of and balance between alkaline salts and albuminous substances in the blood. Recent investigations by Christiansen, Douglas and myself have shown that this concentration and balance are so accurately maintained day by day, and month by month that under normal conditions no deviations can be detected by the most delicate existing method of blood gas analysis. The balance can be temporarily upset by what may be called violent means; but within an hour it is back again to normal. It is, of course, evident that if the carrying-power of blood for CO_2 did not remain normal the breathing and circulation would not, without special adjustment, remain normal.

Now let us look back for a moment, and see where we now stand. The experimental study of the physiology of breathing has led us to the discovery of four normals, the maintenance of which furnishes the interpretation of a mass of what would otherwise be isolated and unintelligible observations. We have first of all the normal alveolar CO_2 pressure. This turns out to be directly subordinate to the normal regulation of the hydrogen ion concentration of the blood, the normal reaction of the respiratory center to hydrogen ion concentration, and the normal regulation of the capacity of the blood for carrying CO_2. With the discovery of each of these normals we have obtained deeper and deeper insight into the physiology of breathing. We have done this, not by merely seeking for causes in the physical sense, but by seeking for interconnected normals and their organization with reference to one another and to other organic normals. These normals represent, not structure in the ordinary physical sense, but the active maintenance of composition. We may fitly call this living structure, since so far as we know all living structure is actively maintained composition, the atoms and molecules entering into which are never the same from moment to moment according to the ordinary physical and chemical interpretation. Our method has thus been essentially the same as that of the anatomist who seeks for the normal—the type—which runs through and dominates the variety of detail which he meets with, and who reaches more and more fundamental types.

I wish, now, to point out that the same method has been applied, and is being applied, to other departments of physiology, even though the physiologists applying it may have failed to realize the far-reaching significance of their results.

I will refer first to the general physiology of the blood. The facts that the hydrogen ion concentration and capacity for carrying CO_2 are very accurately regulated in the blood are no isolated facts in physiology, although the accuracy of our physiological means of measurement renders them peculiarly striking. Claude Bernard, in his *Leçons sur les phénomènes de la vie,* was, I think, the first to point out clearly that the composition of the blood, as well as its temperature, is physiologically regulated. He was led to this conclusion more particularly by his observations that in prolonged starvation there is still sugar in the blood, and that even when great excess of sugar is intro-

duced into the body the percentage in the blood remains very steady, as excess is taken up by the liver and other organs, or excreted by the kidneys. Voit's observations on the relative constancy of the sodium chloride in the blood, and the manner in which the kidneys regulate this percentage, are of a similar character. If food freed from chloride is administered the elimination of chloride by the urine diminishes to almost nothing, though the high percentage of chloride in the blood-plasma remains about the same. As Voit also showed, the blood during prolonged starvation retains its normal composition, and its volume remains proportional to body weight, while other tissues (*e. g.*, muscle) are reduced.

Dr. Priestley and I have recently investigated the excretion of water by the kidneys. By simply drinking large quantities of water one can produce an enormous increase in the secretion of urine, and this urine is almost pure water. What we wished to observe was the degree of watering down of the blood which was necessary to produce the huge increase in excretion of water. We did not doubt that the watering down would be very small, but when we attempted to measure the dilution by determining the percentage of hemoglobin we found that there was no dilution at all, though the method used was one of extreme accuracy. When, however, the plan of measuring the electrical conductivity of the serum was adopted, a slight, but quite distinct, diminution in the conductivity could be detected during, and ending with, the diuresis. This showed that there was a slight diminution in the salt-concentration, and to this diminution the secreting cells were reacting. Here, then, we are in presence of another exactly but elastically regulated normal, the slightest deviation from which produces, in the kidneys, a reaction comparable in its exquisite delicacy with the reaction of the respiratory center or liver or kidneys to a change in hydrogen ion concentration.

The physiology of the kidneys has, in accordance with prevalent physiological conceptions, been attacked from the side of "causal" explanation. I know nothing more hopeless than the attempts to explain the outstanding features of secretion of urine on the lines of ordinary physics and chemistry. So far as the facts are yet known we can, however, get a practical grasp of the kidney activities if we attack the subject from the standpoint of the active maintenance of the normal blood composition.

Let me turn now to the general physiology of nutrition. In the preliminary stages of investigation of this subject physiology has owed much to the pure chemists, and this debt is constantly increasing. We have only to think of the work of such men as Black, Priestley, Lavoisier and Liebig. Like Wöhler, who synthesized urea, Liebig was a convinced vitalist. For him there was a central kernel of vital activity under the control of the "vital force"; but outside this central kernel he interpreted the phenomena of nutrition on purely chemical lines. He thought of oxygen as something free to oxidize anything oxidizable within the body, except what is protected by the vital force; and he assumed that the greater the concentration of oxygen in the lungs, and the greater the supply of food-material to the body, the greater will be the amount of oxidation, since only a limited amount of oxidation is under the direct control of the vital force. He gave special attention to the elimination of urea and other products of nitrogenous oxidation, and introduced methods of measuring the nitrogenous waste. It was found, apparently in direct confirmation of his general ideas,

that the amount of urea excreted rises and falls, except for a certain starvation minimum, in direct proportion to the amount of albuminous food eaten. The excess over the starvation minimum was looked upon as "luxus consumption"—an ungoverned oxidation, due to simple chemical factors.

But the matter was soon carried further by the physiologists—particularly by Pflüger, and by Voit and his pupil Rubner. It was found that, other conditions being equal, the consumption of oxygen is within wide limits independent of the abundance of its supply, and that the actual consumption of oxygen per unit of body weight is very little different during starvation from what it is when abundant food is supplied. In starvation more fat is being oxidized to compensate for the deficiency in albuminous oxidation. Finally, the brilliant work of Rubner established the fundamental fact that within very wide limits different food substances are simply substituted for one another within the organism in direct and exact proportion to the energy which they furnish when broken down. The energy liberation per unit body weight is practically constant, but if excess of food is taken the excess of potential energy is stored up as fat and glycogen, while if food is withheld the stored excess is used up. Even when all the stored fat and glycogen is used up, the organism finally flings its own living structural substance into the balance, and in this last desperate effort to maintain the normal metabolism the nitrogenous oxidation again rises to an amount which for a short time compensates for the energy previously yielded by fat. When death from starvation at length comes the old flag—the flag of life—is still flying.

The massive work of Atwater and his pupils on human nutrition, in which it was shown that the normal daily food requirement of a man is about 3,500 calories in energy-value, was of course a direct extension of the idea of normal nutrition. We maintain an energy consumption of about 3,500 calories, just as we maintain about 5.6 per cent. of CO_2 in our alveolar air, or hemoglobin of 18.5 per cent. oxygen capacity in our blood, or legs of a certain length and anatomical structure. By a strange confusion of ideas the idea is abroad that nutrition is a matter of simple chemistry and physics, and that when we estimate food values in calories, we are exemplifying this fact. This is enough to make a staunch old vitalist like Harvey or Johannes Müller turn round in his grave and laugh. What is it in the body that measures out or withdraws protein, carbohydrate and fat with meticulous accuracy in terms of their energy value, in such amount as to maintain the normal energy metabolism? Is it not the vital spirit or vital force? the old physiologists would ask. Is not this phenomena of a piece with all the other distinctive phenomena of life, and ought not physiology to face these phenomena fairly and squarely and generalize from them, not run away from them? This is the question I am trying to put to you now.

Now I wish to make it clear that it is not vitalism, but simply biology, that I am preaching. Vitalism is a very roundabout and imperfect attempt to represent the facts. Physiological study, and biological study generally, seems to me to make it clear that throughout all the detail of physiological "reaction" and anatomical "structure" we can discern the maintenance of an articulated or organized normal. This idea brings unity and light into every corner of physiology. In other words, it helps us within limits to predict, just as the ideas of unalterable mass and energy help us within limits to predict, or the ideas of time and space help us within limits to predict. I claim nothing more for it, but also nothing less. The idea of life is just

the idea of life. One can not define it in terms of anything simpler, just as one can not define mass or energy in terms of anything simpler. But this one can say—that each phenomenon of life, whether manifested in "structure" or in "environment," or in "activity," is a function of its relation to all the other phenomena, the relation being more immediate to some, and less so to others. Life is a whole which determines its parts. They exist only as parts of the whole.

At first sight it might seem as if it must be very difficult to make use of this conception as an instrument of research: for evidently we can not investigate the parts without investigating the whole. The difficulty is only apparent. The whole is there, however little we as yet comprehend it. We can safely assume its presence and proceed to discover its living details piece by piece, in so doing adding to our knowledge of the whole. If, on the other hand, we attempt to take the organism to pieces, or separate it from its environment, either in thought or in deed, it simply disappears from our mental vision. A living organism made up of matter and energy is like matter and energy made up of pure time and space: it conveys to us no meaning which we can make use of in interpreting the facts.

But is there not matter and energy in a living organism? Do we not assume this at every step in physiology? We make use of the ideas of matter and energy in biology, just as the physicist makes use of the idea of extension in the investigation of matter. To the biologist, however, the structure and activity of an organism are no mere physical structure and activity, but manifestations of life, just as to the physicist the extension of matter is no mere mathematical extension, but a manifestation of the properties of matter, with a physical and not a mere mathematical meaning. This is the answer to those who point to the dependence of physiology on physics and chemistry, and conclude from this that physiology can not be anything but a department of physics and chemistry. By a similar chain of reasoning physics would be nothing but a branch of mathematics, and mathematics itself would melt away into that universe of unconnected "impressions" which David Hume imagined, but Immanuel Kant showed to be nonexistent.

The limits of time prevent my giving further examples of the light which the conception of the normal throws on the details of every part of physiology, and I must now try to probe more deeply. It may be pointed out that although it is useful in matters of detail to bear in mind that a living organism tends to maintain a normal of both structure and activity, and to pass through a normal life history, yet in ultimate analysis all this *must* be due simply to the reactions between its structure and physical and chemical environment. I will not at this point quarrel on general grounds with the "must," but simply endeavor to test it by the facts of physiology.

We can distinguish in a living organism what seems a more or less definite structure of bony matter and connective tissue. Yet we know that all this is built up, and in adult life is constantly being pulled down, rebuilt and repaired, through the activities of living cells. It is thus within the living cells that we must look for the structure which is supposed to react so as to maintain the normal. These cells are made up of what has been called "protoplasm." Now the more we study protoplasm the more evident does it become that this "substance" is extraordinarily sensitive to the minutest changes in environment. Take away or diminish or increase the minute traces of calcium or potassium

salts in the blood-plasma, or the traces of various substances supplied to the blood by other organs; or add traces of certain other substances: the reactions of the protoplasm are quickly altered, and its structure may be destroyed. It is evidently in active relation with its environment at every point, and one can not suspend this activity without altering it. Even deprivation of oxygen for, perhaps, a minute may kill a nerve-cell. There is no permanent physical structure in the cell: the apparent structure is nothing but a molecular flux, dependent from moment to moment on the environment.

Now when we look at the blood, the internal or immediate environment on one side of the cells in the body, we find, as already shown, that this is almost incredibly constant in composition. Were it not so the reactions of the cells would become chaotic, and their structure would be completely altered if not destroyed. But the constancy of the blood is maintained by the combined reactions of the organs and tissues themselves. The intimate structure of the living cells depends on the constancy of the blood, and the constancy of the blood depends on the intimate structure of the tissues. If we regard this condition as simply a physical and chemical state of dynamic balance, it is evident that the balance must be inconceivably complicated and at the same time totally unstable. If at any one point in the system the balance is disturbed it will break down, and everything will go from bad to worse.

A living organism does not behave in this way: for its balance is active, elastic, and therefore very stable. When a disturbance affects its structure or internal environment it tends to "adapt" itself to the disturbance. That is to say its reactions become modified in such a manner that the normal is in essential points maintained. An injury heals up: destroyed tissue is reproduced, or other parts take on its function: the attacks of microorganisms are not only repelled, but immunity to future attacks is produced. In reproduction the body periodically proceeds to renew almost the whole of its structure. Death may be regarded as a periodical scrapping of structural machinery, and reproduction as its complete renewal.

The Anglo-American expedition of which I was a member studied, on the summit of Pikes Peak, Colorado, adaptation to the want of oxygen which causes, in unadapted persons, all the formidable symptoms known as "mountain sickness." As adaptation proceeded the blueness of the lips, nausea, and headache completely disappeared, and it was then found that even during rest the lung epithelium had begun to secrete oxygen actively inwards. The kidneys and liver were now also regulating to a lower degree of alkalinity in the blood, so that the alveolar CO_2 pressure was diminished, and the breathing consequently increased, thus raising the oxygen-supply to the lungs. There was also a marked increase in the hemoglobin percentage and in the blood volume. The organism had so adapted itself as nearly to compensate for the deficiency in oxygen supply, just as a heart gradually compensates for a permanent valvular defect.

The normals of a living organism are no mere accidents of physical structure. They persist and endure, and they are just the expression of what the organism is. By investigation we find out what they are, and how they are related to one another; and the ground axiom of biology is that they hang together and actively persist as a whole, whether they are normals of structure, activity, environment or life history. In other words organisms are just organisms and life is just life, as it has always seemed to the ordinary man to be. Life as such is a reality. Physiology is therefore a

biological science, and the only possible physiology is biological physiology.[3] The new physiology is biological physiology—not bio-physics or bio-chemistry. The attempt to analyze living organisms into physical and chemical mechanism is probably the most colossal failure in the whole history of modern science. It is a failure, not, as its present defenders suggest, because the facts we know are so few, but because the facts we already know are inconsistent with the mechanistic theory. If it is defended it can only be on the metaphysical ground that in our present interpretation of the inorganic world we have reached finality and certainty, and that we are therefore bound to go on endeavoring to interpret biological phenomena in the light of this final certainty. This is thoroughly bad metaphysics and equally bad science. It is the idea of causation itself that has failed, and failed because it does not take us far enough. We have not at present the data required in order to connect physical and chemical with biological interpretations of our observations; but perhaps the time is not far off when biological interpretations will be extended into what we at present look upon as the inorganic world. Progress seems possible in this direction, but not in the direction of extending to life our present every-day causal conceptions of the inorganic world.

I now wish to add a few words as to the relation of physiology to medicine; for I am one of those with an intense belief in the intimate connection between the two sciences, and it seems to me that the mechanistic physiology of the nineteenth century has failed to take the rightful position of physiology in relation to medicine. What is the practical object of medicine? It is to promote the maintenance and assist in the reestablishment of health. But what is health? Surely it is what is normal for an organism. By "normal" is meant, not what is the average, but what is normal in the biological sense—the condition in which the organism is maintaining in integrity all the interconnected normals which, as I have already tried to indicate, manifest themselves in both bodily structure and bodily activity.

Now for the mechanistic physiology there are no interconnected normals, just as in the inorganic world as at present interpreted there are also no interconnected normals. If we look through an average existing text-book of physiology we find a great deal about the effects of this or that stimulus, a great deal also about the external mechanism and chemistry of bodily activity—a great deal, in other words, about what lies on the surface but never takes us further. Along with this there are perhaps also some inconclusive discussions of the possible mechanism of such processes as physiological oxidation, secretion, growth, muscular contraction, or nervous activity. Very little will, however, be found about what in reality lies still more on the surface, but also penetrates deep down—the maintenance within and around the body of normal organized structure and activity. The maintenance of the normal is something for which there is no place in the mechanistic physiology, since according to this physiology maintenance must be in ultimate analysis only an accident of structure and environment—a fitful will o' the wisp which does not concern true science.

But medicine, as we have seen, is supremely interested in the physiological normal. What a man sees at the bedside is

[3] It has been suggested to me that if a convenient label is needed for the teaching upheld in this letter the word "organicism" might be employed. This word was formerly used in connection with the somewhat similar teaching of such men as Bichat, von Baer and Claude Bernard. *Cf.* Delage, L'Hérédité, Paris, 1903, p. 436.

a perversion of the normal, and nature's attempts to restore it, with what assistance medicine can give. For medicine it is necessary to know the normal in its elastic and active organization. He who knows how the body regulates its normal temperature will not confuse heat-stroke with fever, or make the mistake of attributing fever to mere increased heat-production in the body. He who knows how the breathing is normally regulated will be in a position to distinguish at once between various causes of abnormal breathing; and similarly for every abnormal symptom met with in disease. But the mechanistic physiology gives a minimum of information about the regulation of the normal. One looks in vain in physiological text-books for connected accounts of the regulation of breathing, circulation, kidney activity, general metabolism, nervous activity. The main facts of physiology are partly ignored, and partly strewn about in hopeless disconnection and confusion. A student of medicine may learn some true physiology at the bedside, or he may never learn it at all, and become either a hopeless empiric or what I do not hesitate to call a mechanistic pedant.

Medicine needs a new physiology which will teach what health really means, and how it is maintained under the ordinarily varying conditions of environment. We also need a pathology which will teach how health tends to reassert itself under totally abnormal conditions, and a pharmacology which will teach us, not merely the "actions" of drugs, but how drugs can be used rationally to aid the body in the maintenance and reestablishment of health. The new physiology, new pathology, and new pharmacology are growing up around us just now. I can see them more particularly in the splendid advances which the medical and other biological sciences are making in America. You have the advantage of having less of old intellectual machinery to scrap than we have in the old countries; but perhaps we shall not be much behindhand.

If we look on pathology as simply the description of damage to bodily structure, and the analysis of the causes of this damage, then pathology may be very helpful in preventive medicine, but does not help much in therapeutics. When, however, pathology studies the processes of adaptation to the unusual, defence of the organism against the unusual, and reproduction of the normal, just as the new physiology studies the maintenance of the normal under ordinary conditions, then therapeutics and surgery will be aided at every step by pathology, and a rational biological pharmacology will have its chance.

Sometimes one hears the complaint that the world has grown old: that the great discoveries have all been made; and that nothing is left to us now but to work out matters of sheer detail. Perhaps the great and constantly growing mass of rather uninteresting, but otherwise apparently meritorious scientific literature, increases this impression. At certain moments one may long for the past centuries when there was much less to read, and people seemed to have plenty of time to think, and to have endless material for new discoveries and projects. But in reality I do not think that there was ever more scope for new ideas and discoveries than there is at present. Among the new ideas are those of the new physiology, the outlines of which I have tried to trace in this lecture. Those who do not feel inclined to accept this new physiology, or who are still sceptical as to its theoretical basis, will, I hope, at least make allowance for any personal failure on my part to present it to them in a more convincing form.

J. S. HALDANE

NEW COLLEGE,
UNIVERSITY OF OXFORD

THE UNIVERSITY OF ILLINOIS HUDSON BAY EXPEDITION

The University of Illinois geological expedition into the Hudson Bay region during the past summer, which was made possible by a grant from the graduate school, has been completed recently with very successful results.

The primary purpose of the expedition was to make a detailed study of the succession of Paleozoic rocks comprising the great sedimentary outlier west of Hudson Bay, with the object of determining just what formations are represented in that region: a fact of first importance in interpreting the oceanic connections of the ancient epicontinental seas, and the paleogeography of the continent during early Paleozoic time.

Inasmuch as the only source of supplies and provisions throughout a large part of the region is the various fur-trading posts of the Hudson's Bay Company, arrangements were made to outfit through this company at The Pas, Manitoba. The start was made from that place on July 4, the party going as far as Armstrong lake on the new Hudson Bay railroad, and then proceeding down the Nelson river by canoes to the Bay.

Over nearly the entire region bordering Hudson Bay on the west the land is a great muskeg, or swamp, covered with a blanket of peat varying from a few inches up to ten feet or more in thickness. Owing to this fact the country back from the streams is almost impassable in the summer, there being no overland trails except portage paths around rapids in the rivers, or across the low divides from one river system to another. Hence, the party was obliged to travel entirely by canoes.

The exposures of the sedimentary rocks in this region are practically confined to the banks of the larger rivers which, almost without exception, flow across the belt of sedimentary strata. These rocks dip in general towards the bay at a rate a little greater than the fall of the streams, thereby making it possible to obtain a practically complete section of the strata outcropping along each stream. The plan of work was to follow up a river, portage across the divide into the adjacent river basin, follow that down to the Bay, proceed along the coast of the Bay to the next important river, ascend this, cross the divide and follow down the next, etc. In this manner a detailed section of the rocks and a careful collection of fossils were obtained from the Ordovician strata exposed along the Nelson, and Shamattama rivers; from the Silurian rocks along the Severn, Winisk, and Ekwan rivers, and from the Devonian beds, at the south end of the Bay, along the Moose and Abitibi rivers.

Altogether about eighteen hundred miles were traversed by canoes on this expedition, the party reaching the railroad at Cochrane, Ontario, on September 18.

A detailed report containing the scientific results of the expedition will be published as soon as the fossil collections which it was necessary to leave for shipment at the various posts of the Hudson's Bay Company reach the university and can be carefully studied.

T. E. Savage,
F. M. Van Tuyl
Department of Geology,
University of Illinois

SCIENTIFIC NOTES AND NEWS

Dr. Cleveland Abbe, the distinguished meteorologist, died on October 28, at his home in Chevy Chase, Washington, in the seventy-eighth year of his age.

Dr. Wilhelm von Waldeyer, professor of anatomy in the University of Berlin, has been raised to hereditary nobility on the occasion of his eightieth birthday.

A finely illustrated volume, containing thirty-six articles and extending to over eight hundred pages, has been dedicated to Dr. Erik Müller, professor of anatomy at the University of Stockholm, by his friends and pupils on the occasion of his fiftieth birthday.

Professor C. W. Balke, formerly at the head of the division of general chemistry and qualitative analysis at the University of Illinois, is organizing a research laboratory for the Pfanstiehl Company in North Chicago

which is engaged in the application of rare metals to industrial uses.

DR. H. S. ADAMS, of the department of physiological chemistry in the University of Chicago, has accepted a position as research chemist and pharmacologist at the biological laboratories of E. R. Squibb & Sons, New Brunswick, N. J.

DR. WILLIAM GILMAN THOMPSON, professor of medicine in the Medical College of Cornell University, has resigned, and is succeeded by Dr. Lewis Atterbury Conner, professor of clinical medicine in the college since 1905.

DR. ARMINIUS C. POLE, after many years' service as professor of anatomy in the Baltimore Medical College and professor of descriptive anatomy in the University of Maryland since the merger of the two schools in 1913, has resigned.

C. F. HIRSHFIELD, professor of power engineering in Sibley College, Cornell University, who has been absent on leave for special work in Detroit, has resigned.

W. C. PHALEN has resigned his position as geologist in the U. S. Geological Survey and has entered on his new duties as a mineral technologist in the Bureau of Mines, with headquarters in Washington.

PROFESSOR ERNEST BLAKER, of the department of physics, Cornell University, has, on account of illness, been granted leave of absence for the present term.

DR. CHARLES L. PARSONS, of the Bureau of Mines, is in Europe, where he will spend two months visiting plants in connection with the United States work preparatory to constructing a nitrate plant.

DR. HENRY I. ADLER, lately chief of staff of the Boston Psychopathic Hospital, is spending several months in Chicago at the request of the Civic Club and the Illinois Society for Mental Hygiene, under the auspices of the Rockefeller Foundation. His especial work is to be a survey of the mental defectives of Chicago and Cook County, and he will work in the courts and other institutions where there are facilities for detecting and handling defectives.

DR. CLARK WISSLER, of the American Museum of Natural History, during the summer, continued his work with Mr. James R. Murie, chief of the Pawnee Indians of Oklahoma. With the aid of Mr. Murie, Dr. Wissler has secured many interesting rituals of the religion of the Pawnee, which is now passing away. The more important parts of these rituals have been written down as texts in the Pawnee language with translations in English.

DR. FRANK E. LUTZ, of the American Museum of Natural History, and Mr. J. A. G. Rehn, of the Academy of Natural Sciences of Philadelphia, spent part of the summer studying and collecting insects in the vicinity of Tucson, Arizona. Mr. B. Preston Clark generously contributed toward the field expenses and the Philadelphia Academy also cooperated in the work. In addition to securing specimens for the study collection, an effort was made to obtain material which would bear especially upon the problems of ecological and geographical distribution.

AT the opening exercises of the College of Medicine of the University of Illinois, held in Chicago, on October 5, Edmund J. James, president of the university, delivered an address on the "Function of the State in the Promotion of Medical Education and Research."

AT the annual meeting of the American Roentgen Ray Society, held in Chicago the last week in September, Professor W. S. Miller, of the department of anatomy at the University of Wisconsin, delivered by invitation an illustrated address on "The Architecture of the Lung and its relation to the Proper Reading of X-ray Plates."

PROFESSOR BIRD T. BALDWIN, of Swarthmore College, has been appointed lecturer in education at the Johns Hopkins University. He is giving, on Saturdays, a course on "Educational Measurements," continuing the special work he began in the university's summer session.

DR. PERCIVAL LOWELL, director of the Lowell Observatory, Flagstaff, Ariz., left Boston on September 27 for an extensive astronomical

lecture trip. He is speaking at the State College of Washington, University of Washington, Reed College, Oregon Agricultural College, University of Oregon, Leland Stanford Junior University and the University of California. Before returning he will spend some time at his observatory in Flagstaff.

PROFESSOR EMMANUEL DE MARTONNE, professor of geography in the Sorbonne, arrived in New York on September 18 to take up his work as visiting French professor at Columbia University. He is giving courses on European physiography under the auspices of the department of geology. His offerings include two courses of four lectures each, delivered in French and open to the public. The subjects and dates of these lectures are: (1) Montagnes du Centre et Sud de la France, 4:15 P.M. (Massif Central), October 19 and 26, November 2, and (French Alps) November 9; (2) Plaines et Champs de Bataille du Nord de la France, 8:15 P.M., November 15, 22 and 29, and December 6. In connection with this series of lectures, there are conferences, open to advanced students, in which a detailed study of certain phases of the work will be made. Professor de Martonne is also cooperating with Professor D. W. Johnson in a course on the physiography of Europe, in which the Alps, the Carpathians, and southeastern Europe will be discussed by Professor de Martonne.

THE New York sections of the American Electrochemical Society and the Illuminating Engineering Society have arranged for a joint session to be held at the Engineering Societies Building, 29 West 39th St., New York, on Thursday evening, November 9, at 8 o'clock. A program has been prepared including papers on "High Pressure Gas Installations," "The Chemistory of Gas Lighting" and "The New Flexible Mantle." Engineers and chemists interested are cordially invited to attend.

THE Thomas Hawksley lecture of the British Institution of Mechanical Engineers was delivered by Mr. H. E. Jones, on November 3, on the subject of "The Gas Engineer of the Last Century."

WE learn from the *British Medical Journal* that at the recent general meeting of the Medical Society of London, the retiring president, Sir St. Clair Thomson, drew attention to a plaque removed from the society's house in Bolt Court to the present library. The plaque was erected originally by Dr. John Coakley Lettsom, the founder of the society. The incoming president, Lieutenant-colonel D'Arcy Power delivered an address on "John Ward and His Diary." The Lettsomian lectures will be delivered by Colonel Cuthbert Wallace, C.B., and the oration by Sir William Osler.

DR. LOUIS MCLANE TIFFANY, emeritus professor of medicine at the University of Maryland, and consulting surgeon for the Johns Hopkins Hospital, died on October 23, at seventy-two years of age.

PICTURES of surgery done by Dr. Alexis Carrel and others on the wounded soldiers in French hospitals have recently been made with a cinema camera and brought to this country by the Clinical Film Company. The picture will be shown before medical societies and medical students.

UNIVERSITY AND EDUCATIONAL NEWS

THE General Education Board has announced the following appropriations: Albion College, Albion, Mich., \$100,000; George Peabody College for Teachers, Nashville, Tenn., \$200,000; Hamline University, St. Paul, Minn., \$100,000.

ISAAC F. NICHOLSON, Baltimore, celebrated his eightieth birthday by giving the Johns Hopkins University \$15,000 for the establishment of the Isaac Forester Nicholson Fund, to establish scholarships for needy students from Baltimore or the state of Maryland, or to be used for any other purpose the trustees may desire.

BISHOP CANDLER, chancellor of Emory University, announces the receipt of a contribution of \$50,000 from J. J. Gray, Jr., Rockdale, Tenn., for the erection of an outpatient building in connection with the medical department

of Emory University, the building to be known as the J. J. Gray Clinic.

ORSON BENNETT JOHNSON, professor emeritus of zoology in the University of Washington, has given the university his valuable entomological collection.

DR. J. ERNEST CARMAN, of the University of Cincinnati, has been appointed to the chair of geology at the Ohio State University vacant by the death of Professor Charles S. Prosser.

DR. JULIUS H. HESS has been appointed professor of pediatrics and head of the division of pediatrics in the University of Illinois, college of medicine.

DR. FRANK MALTAUER, formerly of the Cincinnati Board of Health, has become associate professor of bacteriology and public health at the College of Medicine, University of Tennessee.

DR. ALBAN STEWART, instructor in botany at the University of Wisconsin, has been appointed professor of botany and bacteriology in the Florida State College for Women, Tallahassee, Florida.

DR. R. L. BORGER, of the University of Illinois, has been appointed professor of mathematics at Ohio University, Athens, Ohio.

DR. EDWARD HART has retired as active head of the chemical department of Lafayette College, but remains connected with the department as professor of chemical engineering and as librarian of the Henry W. Oliver Chemical Library. Dr. Eugene C. Bingham has resigned the professorship of chemistry at Richmond College to become professor of chemistry and director of the Gayley Laboratory at Lafayette College. Last year Dr. Bingham was on leave of absence from Richmond College in order to carry out some special investigations at the Bureau of Standards on the subjects of fluidity and plasticity. Dr. J. Hunt Wilson, of Lehigh University, has become assistant professor of chemistry at Lafayette College.

J. F. WILSON, formerly instructor in electrical engineering at the University of Michigan, has been appointed professor of electrical engineering at Queen's University, Kingston, Canada, to take the work of Professor L. W. Gill, while the latter is in active military service.

DISCUSSION AND CORRESPONDENCE

SUNLIGHT AND THE MAGNETIC NEEDLE

THE editorial page of the *Electrical World* for April 1, 1916, contains the following paragraph pertaining to the subject of magnetism and terrestrial magnetism:

> Considering how many centuries have elapsed since magnetic phenomena first became recognized on this planet, it is remarkable how little has yet been learned concerning the nature and laws of magnetism. All that we are able to affirm, with a reasonable degree of certainty, is that whatever electricity and magnetism may be, they must be so interrelated that one is the consequence of the curl of the other, which is one aspect of Maxwell's electromagnetic theory.
>
> As an instance of our magnificent international ignorance of the nature of terrestrial magnetism, the simple historical fact may be cited that in 1582, the date of the international introduction of the Gregorian Calendar, with a sudden jump of ten days, the magnetic needle at London pointed 11 degrees easterly of the geographic meridian, whereas it now points nearly 16 degrees westerly of that meridian, and in 1820 nearly attained 25 degrees of westerly declination, a total of more than 36 degrees, while no satisfactory theory of the large change has yet been produced.

The foregoing is particularly interesting to the writer, who is directly interested in collecting ocean data on the non-magnetic ship *Carnegie* to be used, first, practically in constructing charts for navigation and, second, in theorizing on the causes of the earth's magnetism and on its changes as referred to. I desire to call attention to the work of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, D. C., in the making of extensive magnetic observations leading to the formation of some correct theory of the causes of the earth's magnetism.

The writer wishes to contribute the following, on the general subject of magnetism, of whatever value it may be.

So far as I am aware neither Faraday in his experimental researches nor Maxwell in his mathematical treatment thereof, nor any one else recently, ever proposed or performed an experiment, excepting the experiments with polarized light, to show that a direct connection existed between light and magnetism.

At the end of Faraday's first period of brilliant discoveries or about 1841 various investigators[1] had performed many experiments with this end in view.

In general these had taken the form of attempts to magnetize bodies by exposure in particular ways to different kinds of radiations; and a successful result had been more than once reported only to be proven in error on reexamination.

Sir John Herschel was the first to indicate the true path of procedure. He wrote:

> Induction led me to conclude that a similar connection exists, and must turn up somehow or other, between the electric current and polarized light and that the plane of polarization would be deflected by magneto-electricity.

Faraday had already discovered the nature of this connection in 1834, but had considered his experiment a failure. In 1845 after Herschel's remark he varied the original experiment, with success, by placing a piece of heavy glass between the poles of an excited electromagnet; and found that the plane of polarization of a beam of light was rotated when the beam passed through the glass parallel to the magnetic lines of force composing the field. This constituted the discovery of the connection between light and magnetism.

In 1851 Faraday wrote:

> It is not at all unlikely that if there be an *æther*, it should have other uses than simply the conveyance of radiation.

This sentence has been considered the origin of the electro-magnetic theory of light.

The question which natural philosophers had never ceased to speculate on, that of the manner in which electric and magnetic influences are transmitted through space, assumed a definite form about the middle of the nineteenth century and issued in a rational theory. It was at this point that the whole matter was taken up and eventually theoretically solved by Maxwell. He said:

> We can scarcely avoid the inference that light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena.

At the time Maxwell did not examine whether this relation was confirmed by experiment. For years the electromagnetic theory was beset with difficulties and was unfavorably received by his most famous contemporaries. Helmholtz after many years accepted it, but Lord Kelvin, it seems, never did.

It is quite interesting to note here that Lord Kelvin in 1904 admitted that a bar magnet rotating about an axis at right angles to its length is equivalent to a lamp emitting light of period equal to the period of rotation, giving his final judgment, however, that "the so-called electro-magnetic theory of light has not helped us hitherto."

While pondering over the subject of terrestrial magnetism, electricity and magnetism on the night of Tuesday, March 7, 1916, the following thought came to me with such force that I set it down in my diary. A copy is as follows:

> I conceived the idea to try the effect of a concentrated sunlight on the magnetic needle or magnetized bar of any kind. The question being will not the concentrated light lessen or strengthen the magnetism of the magnet?

In performing such an experiment arrangement must be made so as to exclude the effects of the absorbed energy appearing as heat. I intended to try this as an experiment at some convenient time in the hopes that some new connection might be brought about concerning the subject of light, electricity and magnetism and their mode of propagation.

On Saturday, March 11, 1916, four days afterwards, I chanced to see a newspaper clipping regarding some work of Professor T. J. J. See, of Mare Island, Cal. In this article Pro-

[1] Morichini, of Rome, 1813, *Quart. Journal of Science*, XIX., p. 338. S. H. Christie, of Cambridge, 1825, *Phil. Trans.*, 1826, p. 219. Mary Somerville, 1825, *Phil. Trans.*, 1826, p. 132.

fessor See proposed to explain many things, among them being "the direct effect of sunlight on a magnetic needle, as in Nipher's experiment of 1913." This was a complete surprise. Evidently this experiment had been tried with success by I suppose Francis E. Nipher, of Washington University, St. Louis, Mo.

It seems to me that such an experiment would be valuable to science in many ways. The question arises as to the quantitative effect produced—if appreciable, then might we not expect or predict a change in all magnets more or less with time—especially as they are exposed to the sunlight? It is well known that magnets lose some of their magnetism during the process of ageing. Might this effect be a contributing cause?

The question as to the effect on small magnets such as in use for the determination of the earth's magnetic elements assumes some importance when considered in this regard.

What might be the effect of the sunlight on the magnet if it were rotated about a horizontal line through its center of mass and perpendicular to its magnetic axis? The theory of magnetization by rotation has been treated in two articles appearing recently in Science by Barnett.

Aside from the foregoing it would be interesting to note the effect, if any, of radioactive emanations upon a magnetic needle.

There are two well-known cases of the transformation of luminous into electrical energy, the thermopile and the photo-electric cell. However, in neither one is the transformation direct, as would be the case of luminous energy falling upon the magnetic needle.

It would be interesting to see this matter investigated in the light of modern electrical theory and to know of Nipher's experiment and of the results obtained.

F. C. Loring

Department of Terrestrial Magnetism,
Carnegie Institution

GUMBOTIL, A NEW TERM IN PLEISTOCENE GEOLOGY

The term gumbo has been used for many years by some geologists in America for a dense, impervious clay, which, when saturated with water, is sticky and tenacious. The name has had no relation to the origin of the material: in many cases it has been applied to alluvial deposits on the flood plains of streams: McGee, Leverett and others have applied it to a gray to drab-colored clay overlying drift, the origin of the gumbo having been attributed to various causes, some having considered it to be, mainly, of fluvio-glacial origin, others to be aqueous, and still others have thought it to be related to loess.

In a recent paper in volume 27 of the Geological Society of America, pages 115 to 117, the writer discussed a gumbo which lies on Kansan drift and which he had studied in considerable detail in southern Iowa. This gumbo is limited in distribution to tabular divides and other remnants of the Kansan drift plain. The view was there expressed that the field evidence suggested strongly that the gumbo is the result, chiefly, of the chemical weathering of Kansan drift. It was stated, also, that detailed chemical analyses of the gumbo and the underlying materials were being made by Dr. J. N. Pearce, of the chemistry department of the University of Iowa, to ascertain whether the analyses would strengthen or weaken the interpretations made from the field evidence. These analyses have now been completed and will soon be published. They seem to show clearly that the gumbo is the weathered product of the drift.

During the present summer, the writer has extended his studies into the western, northwestern and northern parts of Iowa, and at scores of places sections have been examined which show clearly the intimate relations between the gumbo and the underlying Kansan drift. Moreover, it is of interest that in many places a gumbo has been found on the Nebraskan drift, the relations of the gumbo to this drift being similar to those of the super-Kansan gumbo to the Kansan drift. Furthermore, after a somewhat careful study of the gumbo which lies on the Illinoian drift in southeastern Iowa, and which has been discussed by Leverett in Monograph XXXVIII. of the United States Geological Survey,

pages 28 to 33, the conclusion has been reached that here, also, the gumbo is so related to the drift that it is undoubtedly the thoroughly weathered product of the Illinoian drift.

As a result of the field investigations and the chemical studies it is now proposed that the somewhat indefinite term "gumbo" be no longer used for these super-drift clays, but that the name "gumbotil" be used. Gumbotil is, therefore, a gray to dark-colored, thoroughly leached, non-laminated, deoxidized clay, very sticky and breaking with a starch-like fracture when wet, very hard and tenacious when dry, and which is, chiefly, the result of weathering of drift. The name is intended to suggest the nature of the material and its origin, and it is thought best to use a simple rather than a compound word. Field work has already established the fact that in Iowa there are three gumbotils, the Nebraskan gumbotil, the Kansan gumbotil and the Illinoian gumbotil.

GEO. F. KAY

UNIVERSITY OF IOWA

THE EVOLUTION OF HERBS

THE article by Edmund W. Sinnott, published last week in SCIENCE, *44*: 291, supports conclusions on this subject arrived at from quite a different standpoint.

The idea that trees are primitive forms is involved in the proposition advanced by Henry L. Clarke, in the *American Naturalist, 27*: 769-81, September, 1893, that in their order of blooming the generalized precede the specialized.

My observations were based only on entomophilous flowers, 493 native and 61 introduced.

If we assume that the earliest, least specialized, and primitive plants form the earliest maxima and succeed in regular order, we shall have for indigenous plants the following results according to the time of the maxima:

Trees	April 27–May 8
Woody climbers	June 13–15
Shrubs	June 21–23
Perennial herbs	August 2–6
Annuals and biennials	August 30–September 6

And this seems to be the probable order of their development. The original plants having the most freedom developed large size and occupied the most favorable positions. The less favored could become reduced to shrubs and finally to herbaceous perennials, and occupy many positions which were unfavorable for trees or with which trees did not interfere. The habits of perennial herbs are better understood if we suppose that they had to compete with trees, or rather avoid competition with them, from the first. The annuals developing later were able to find many temporary situations unfavorable for woody plants or perennial herbs. The primitive Angiosperms were probably trees, like Magnoliaceæ, Anonaceæ and Lauraceæ.

Another general characteristic of blooming seasons is that the earliest, most generalized, most primitive plants have the shortest seasons, while the most specialized, most recent, and latest arrivals have the longest seasons. Arranging the vegetative forms according to their average blooming seasons, we have the following order:

	Days
Woody climbers	36.5
Trees	39.4
Shrubs	42.7
Perennial herbs	57.1
Annuals or biennials	75.1
Cosmopolitan	80.4
Introduced	117.3

Except for trees and woody climbers, the order is the same as for the maxima.

CHARLES ROBERTSON

CARLINVILLE, ILL.,
September 6, 1916

HORSE FLESH AND THE DIET OF EARLY MAN

TO THE EDITOR OF SCIENCE: In SCIENCE, for September 22, is published a letter on the "Animal Diet of Early Man," which discusses the subject with reference to possible evidence drawn from tapeworms and their hosts. In this connection, the writer of the letter speaks of the horse as food, as follows:

There is nothing to show that horses were not eaten, unless the rather widespread abhorrence of eating horse flesh at the present time can be con-

strued that man never adapted himself to that diet as he did to beef.

It is worth recalling that any such prejudice in European races is only a thing of yesterday, when discussing such a question as this, since horse flesh was eaten in parts of Europe at least for an apparently unlimited time. It went out of use when it was declared "unclean" by Pope Gregory III., who died in 741. This is discussed in a paper by Esser, on horse flesh, which appeared in the *Journal für Landwirtschaft*, 43 (1895), No. 3, pp. 349–358. The prohibition was so effective that horse flesh did not assume importance in Europe again until after 1870.

C. F. LANGWORTHY
U. S. DEPARTMENT OF AGRICULTURE

ANOTHER TYPICAL CASE

TO THE EDITOR OF SCIENCE: About a year ago a short article by Professor Pickering appeared in SCIENCE under the heading "A Typical Case." The point of the discussion was that a man who had been trained to a high technical efficiency in research had been obliged to take a position in which he was overworked and underpaid to such an extent that he had been forced to give up research because of lack of time and funds, particularly the latter. Thus the world at large loses the benefit of his experience and training.

I am personally interested in a closely related problem which I would like to have considered. I can illustrate it best, probably, by some account of my own experience and I am going to put it on a frankly personal basis, so that due allowance may be made for my own feeling in the matter. My first acquaintance with research was in some preliminary work on a problem in morphology. At that time I was on a fellowship stipend. Marriage at the end of the year made it impossible to continue on such a condition. In connection with high school teaching the line of study was shifted to a rough biological survey of the locality. This was interrupted by a shift in location and the next opportunity for advanced study happened to be in the line of history. A little later the unfortunate acceptance of a position with a bankrupt college caused me to be stranded in the middle of the year and I again took up my original problem in morphology. This study was advanced sufficiently by the end of the year to enable publication of a paper which received favorable comment from workers in that line, especially abroad. Overload in teaching for the next few years prevented any systematic research being done. Finally an opportunity came for attendance at another university, expenses being partly met by acting as half-time assistant. The results of research of that year were covered by a paper on regeneration. Since that time I have not been able to command sufficient funds to enable me to attend regular sessions of a university and support my family. I have had some summer-school study but not of a sort to give residence credit, I am informed.

For three and one half years now I have been working on a local plankton problem under the advice and direction of a university authority on that subject. For more than two years of that time I have averaged more than fifteen sixty-minute periods per week through fifty-two weeks of the year in study of quantitative and qualitative features of the problem. The value of half of that time has been at least trebled through the aid given by my wife in computing and recording. I am hoping to get my own paper into press this year. I have not been able to obtain any university credit for this work because it could not be counted "in residence." I would like to have a Ph.D. degree, because it seems that that is regarded as a necessary factor in finding a position which would enable me to support my family and still carry on the research in which I am so much interested. Since my efforts have almost exhausted my scanty resources, such a point is of very great interest to me.

I feel quite certain that my case is fairly typical in much the same way as the one mentioned by Professor Pickering. It may be that I have actually done more research than some of similar experience but there are individuals who have done more than I have. There are also a good many who could do acceptable research but who get off in small communities

without any chance of stimulus by personal contact with investigators and so allow their interest to die.

It is becoming more and more certain that amongst other bad features of our "educational system" there is a growing tendency to formation of caste distinctions. The high-school teacher to some extent assumes an exclusive air toward the grade teacher, the university teacher toward the high-school teacher. Within the universities teachers without doctor's degrees sometimes find an embarrassing attitude among their own fellows. A newly fledged doctor sometimes considers himself superior to an older man with a lower degree.

If the universities can not possibly grant higher degrees for extramural work, no matter how valuable, may it not be possible to devise a method by which recognition and encouragement may be given to those doing effective research not technically recognizable under university rules. Could a national council be assembled to confer some mark of merit upon such people? Could a society somewhat like Sigma Xi be formed for such a purpose? If something could be done and a high but reasonable standard maintained, a man with such recognition might stand as high or even higher amongst scientists than the mere doctor. For is not achievement in the face of adversity of greater value than achievement with every facility granted? Is not the man who can do much with little better than the man who must have much in order to do at all?

I am perfectly aware that the easy-chair type of university man will sneer at such a proposal, but I feel sure that there is truth in my contention. I know some one will say that proper research can not be done with poor equipment. Much of the finest research ever done in any and every line has been done with poor equipment and such things might happen again. The man with poor equipment sometimes makes up in resourcefulness for far more than the fine equipment that another may have. Then too there are many problems yet to be solved which do not demand expensive or elaborate equipment.

I also anticipate the objection that standards would be hard to fix or sustain for such recognition as I have suggested. The results could scarcely be worse than they are for the doctor's degree. I know one state superintendent of public instruction who flourishes a Ph.D. without ever doing any graduate work. In another case a man boasts of the way in which he manipulated credits through two of our best known universities so as to get the degree in two years. In two cases I have heard about the thesis for the degree was repudiated by the department in which the work was done almost as soon as published. A national council would certainly do no worse than this.

So far as I am personally concerned I am determined to go on with such research as I can whether I get any sort of recognition or not, but my own situation has made me think deeply on the matter and I have finally concluded that something could be done to at least encourage isolated workers if scientific leaders cared to do so.

W. E. Allen

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SCIENCE IN THE SERVICE OF THE NATION

The suggestion contained in *The Scientific Monthly* for September, 1916 (p. 310), that the National Research Council's proposal to help "render the United States independent of foreign sources of supply liable to be affected by war," but failure to propose anything looking toward the cooperation of our nation with other nations in producing supplies, might not meet the approval of all scientific men, is well taken.

That science is in for a period of criticism, even condemnation, because of the part it is playing in the modern war game is indicated by mutterings to this effect heard in diverse quarters. How is the charge to be met?

The mere pointing to what science can do through medicine and other instrumentalities to relieve somewhat the horrors and destruction of war, is clearly not enough. Something more than repair work is needed.

So universal and impersonal are the principles and methods with which science works,

and so fundamental to it are correlation and cooperation, it does seem that among its proposals of service the National Academy of Sciences might include something looking toward the improvement of international relations.

For instance, has science nothing to contribute to the supreme international problem of the day, that of the use of the high seas? And can science suggest no way of utilizing its riches of anthropological and psychological knowledge through governmental channels to help toward a better understanding among peoples of different nations and races?

Lack of sympathetic knowledge on the part of citizens of one country about those of other countries is undoubtedly one of the fertile sources of international friction and hatred; and since a nation must have a large measure of responsibility for its nationals while sojourning in foreign lands, it seems only reasonable that it should make some effort to prevent its citizens, especially those engaged in international trade, from needlessly imperiling its good relationships with other nations.

Since such knowledge is so largely involved in ethical science which in turn is inseparable from physical and cultural anthropology and comparative psychology, it would seem eminently proper that a National Research Council created at the request of the President of the United States falls short of recognizing its full possibilities if it has nothing to propose touching these vital aspects of the national life.

WM. E. RITTER

THE SCRIPPS INSTITUTION FOR BIOLOGICAL RESEARCH OF THE UNIVERSITY OF CALIFORNIA,
September 22, 1916

QUOTATIONS

SCIENCE AND INDUSTRY

ON July 28, 1915, an Order in Council constituted two new bodies—a "Committee of the Privy Council for Scientific and Industrial Research," of which Lord Crewe (as Lord President) is chairman, and an advisory council, consisting of eight very eminent men of science under the chairmanship of Sir William McCormick. The first annual report of each of these bodies is now published, and that of the latter, signed by Sir William McCormick, is a document of considerable length and importance. He and his scientific colleagues have made a serious attempt to gauge the extent of our deficiency, both in the volume of scientific research which is being conducted in this country and in its correlation to the needs of industry. In reviewing the question they recognize that the distinction between "pure" and "applied" science is, in a sense, a false one. They point out that all the important advances which recent generations have made in industrial science, from wireless telegraphy to synthetic indigo, have been the direct outcome of discoveries made by "pure" science conducting research solely for its own sake. At the same time they have temporarily concentrated their first attention upon "research of directly industrial application," both for reasons of industrial urgency and because the universities, which are the natural homes of research in pure science, have been so depleted both of students and of teachers by the war, that "they are barely able to continue their routine work, and can command at the moment neither the leisure nor the detachment of spirit that are essential conditions of original research."

Within this narrower field their first step was to save from actual or imminent abandonment a number of researches which were being conducted or directed by professional associations in the period preceding the war. These have been kept going by a series of government grants, and in one case by getting the War Office to release the investigator from military duties. The next step was to hold conferences with the various professional societies and trade associations. These showed that in the main it is the most highly organized industries that have made most use of scientific research, and are therefore most ready for, though perhaps not most in need of, encouragement to make more. Thus "the engineering trades, with their attendant group of distinguished professional societies, have

long been alive to the need and value of scientific research; while the chemical trades for the most part are so divided and individual in outlook that the various professional societies have had neither the influence nor the means necessary to enable them to take any large share in promoting research in connection with those industries." Simultaneously the council undertook a very important project—the formation of a register of all researches actually being conducted on the outbreak of the war. They have also been busy in forming standing committees to advise them on special subjects. One on metallurgy, one on mining, and one on engineering have been set up, with sub-sections of each; and others are in contemplation. The question of aid to research in educational institutions is likewise engaging them. It will thus be seen that they have broken a good deal of ground during the year. But their main task is still ahead; and on this they make some careful observations.

There are two aspects of it—on the one hand, the sheer deficiency of scientific research and training in the country, and on the other, the failure of manufacturers to appreciate the conditions under which science can help them. The first is to some extent a quantitative matter, upon which an increase of endowments can do a great deal. The second is very intricate, since there is no problem of industrial structure—whether the relation of firms to other firms, or that of firms to their employees—which has not its bearing upon it. The council point out that a state of things, under which a number of relatively small firms in a country are more bent on cutting each other's throats than on promoting the success of the national industry against organized foreign competition, can rarely if ever be conducive to scientific advance in an industry. A certain amount of willingness to pool researches and results is almost indispensable to such an advance; and the more there is, the more advance can be hoped for. Quoting a famous American example the council distinguish three sorts of laboratories which a trade requires: (1) An ordinary works laboratory, such as a firm needs for routine tests and controls; (2) an "efficiency" laboratory, studying improvements in products and processes; (3) a laboratory devoted to more fundamental research, whose fruit is less immediate, though over long periods it will prove supremely important. Only a very large firm or else a combination of firms can be expected to undertake all three; and thus the future of industrial science is very closely linked to that of industrial combination. Another factor upon which the council lay hardly less stress is that of solidarity between firms and their employees, such as only a thoroughly generous and enlightened treatment of the employees can secure. It is not an accident that the firms which have been most conspicuous in the world for their scientific advances—such as the Carl Zeiss firm of Jena—have also been most conspicuous for enlightened and generous conditions of employment. The connection between "welfare work" and a more scientific industry is close and vital.—*The London Daily Chronicle.*

SCIENTIFIC BOOKS

The Turquoise. A Study of its History, Mineralogy, Geology, Ethnology, Archæology, Mythology, Folklore and Technology. By Joseph E. Pogue, Ph.D. Third Memoir, Vol. XII., National Academy of Sciences, Washington, D. C., 1915. Pp. 162. 22 plts. 4to.

While not ranking in intrinsic value with the precious stones *par excellence,* diamond, ruby, sapphire and emerald, no gem-material has longer enjoyed favor for personal ornament than the beautiful turquoise. Three thousand years before the beginning of our era, the Egyptians adorned their jewels with turquoise from the mines of the Sinai Peninsula, from very ancient times the famous Persian deposits at Nishapur have yielded material of the finest quality to the Orient, and in our own land, for the aborigines of the southwest and for the Aztecs of Mexico, the turquoise was at once a gem of exceptional beauty and one to which they attributed talismanic powers.

Hence it is that no more attractive subject for a monograph can well be imagined than the history and study of the turquoise, and

specialists as well as general readers are to be congratulated that this subject has now been adequately treated in all its manifold aspects by one so thoroughly qualified for the task as Dr. Joseph E. Pogue. The writer has disposed his material very systematically and logically. The first chapter (pp. 9–22) is devoted to the history of the stone and embraces a series of citations from early writers, both classical and Oriental, in chronological order. This is followed by a short chapter on the mineralogy of the stone (pp. 23–27). The localities where turquoise has been found are enumerated and fully described in the next chapter (pp. 28–59). To the geological side of the subject is devoted the fifth chapter, on the origin of turquoise. The four remaining chapters deal, respectively, with the use of turquoise (pp. 68–104), the chalchihuitl question (pp. 105–109), the mythology and folklore of turquoise (pp. 110–128), and the technology of turquoise (pp. 129–136). There is also a very copious bibliography, embracing over a thousand titles (pp. 137–154), and an excellent index (pp. 155–161).

The turquoise mines of the Sinai Peninsula, the oldest in the world, were worked from about the time of the I. Dynasty (about 4500 B.C.)[1] to the reign of Rameses VI. (1161–1156 B.C.), since which time turquoise does not appear to have been much used in ancient Egypt. The ancient mines in the Wady Maghara were rediscovered in 1845 by Major MacDonald, a British cavalry officer. The Egyptian name may have been *Mafek* or *Mafkat,* although this word appears rather to have designated malachite and other green stones. The writer of the present notice has conjectured that the *shoham* stones of the breastplate and on the shoulders of the Hebrew high priest may have been turquoises.[2] Strange to say a similar uncertainty hangs over the question whether Pliny's *callaina* means malachite or turquoise. Here again, although Pliny apparently wishes to describe a green stone, the word or a variant (also used by Pliny) *callais* came to mean the stone later called turquoise. A very probable conjecture accepted by Dr. Berthold Laufer, is that Pliny's sky-blue jasper (jaspis aerizusa) is the turquoise.[3] As an aid to the study of the early mentions Dr. Pogue has given a great number of passages referring to the turquoise, from classical and Oriental writers, in translation, although we must bear in mind that in some cases the English rendering "turquoise" is not certainly the meaning of the foreign original. The earliest use of this name, signifying that the stone was brought by way of Turkey to western Europe, is in the Latin gem-treatise of Arnoldus Saxo, written in the early part of the thirteenth century.

In the New World, among the Aztecs, the name *chalchihuitl* seems to have been applied to both green and blue stones, as with the other designations we have noted, and undoubtedly some chalchihuitls were turquoises. Of its use in decoration by the ancient Mexicans, certain curious masks, inlaid with this stone, offer incontrovertible evidence. The finest of these are in the Christy Collection of the British Museum (see Plate 15 of Dr. Pogue's book). Full descriptions are also given of typical turquoise-incrusted or decorated ornamental objects and jewelry made in later times by the Pueblo Indians and by the Navajos of Arizona and New Mexico.

The details as to turquoise mining in our day at the old Nishapur deposits are very interesting and valuable (pp. 37–39). The output is carefully classified into three categories, the first-class material, being called *Angushtari,* literally, "ring stones"; large pieces of this have brought as much as $1,500, and pieces no

1 The uncertainty as to the exact initial date of the I. Dynasty is shown by the difference in the figures given by leading Egyptologists. The latest date is that of Lepsius, 3892 B.C. Then come Brugsch Bey with 4400 B.C., Flinders Petrie with 4777 B.C., and Mariette with 5004 B.C. Champollion, the father of Egyptology, even gave 5867 B.C., as the opening date. Brugsch Bey states that turquoise was already mined in Egypt in 4000 B.C., during the III. Dynasty, at the time of King Snefru, and that mining was not carried on later than the reign of Rameses II., 1300 B.C.

2 George Frederick Kunz, "Curious Lore of Precious Stones," Philadelphia and London, 1913, p. 299.

3 Pogue's "The Turquois," p. 11.

larger than a pea may be worth $40. Each stone is separately and accurately appraised. The second-class material, *Barkhaneh,* is sold by weight, bringing at the mines from $25 a pound for the poorer quality, up to $450 for the best quality. The third class, *Arabi,* is only utilized in Asia, for inlaying, incrustation, and so forth, a lot of twelve pounds once bringing only $300. In the United States a mine in the Cerrillos district, New Mexico, is believed to have produced more fine turquoises than any other deposit, the finest specimens being only equalled by some from the Burro Mountains in the same state, and from Nevada.

Within the narrow limits of this notice we can only touch upon a few points suggesting the wealth of carefully selected and excellently arranged material that Dr. Pogue has so indefatigably assembled here. For ethnologists and students of folklore, the chapter on the mythologic and talismanic fancies connected with this "celestial stone" among many different peoples, will prove especially interesting and instructive. The many plates are well selected to illustrate the subject and are clearly and effectively printed.

Certainly no one who acquires this book will fail to find it all, or more than all, that he expected, and we think that the thanks of those interested in the subject are due to the National Academy of Sciences and to the scholarly author, for having thus enriched our precious-stone literature.

It is very rarely that all the citations relating to a given subject are quoted in extenso, giving the exact and full reference. To the student and scientific worker this is of inestimable value, because frequently when only partial quotations are made, and the references are even inaccurate, much time is consumed in searching for an item which it is almost impossible to locate. What a great assistance it is, particularly to delvers in scientific fields, when, without loss of time in going from one library to another, all the data on a certain subject are found under one cover and immediately at hand. This has been made possible through the far-sighted policy of the National Academy of Sciences, and is especially exemplified in their publication, Volume 13, a catalogue of the Meteorites of North America, dated January 1, 1909, by Oliver Cummings Farrington. These two memoirs, in the presentation of their rich references with the deductions of experienced workers, are noteworthy contributions to two subjects, than which there is probably none of greater interest to the archeologist, petrologist, chemist, student and general worker.

George F. Kunz

The Mythology of All Races. In thirteen volumes. *North American.* By Hartley Burr Alexander, Ph.D., Professor of Philosophy, University of Nebraska. Volume X. Marshall Jones Company, Boston, Mass. 1916. Pp. 325, 23 full page and 2 text illustrations, linguistic map, 45 pp. Notes, 11 pp. Bibliography, authorities used.

Volume X. is one of the two volumes recently published of a series, the purpose of which, as stated by the editor, Dr. Louis Herbert Gray (Vol. I., p. xii), is to assemble "into a single unit" the mythologies of all races and "since the series is an organic unit—not a chance collection of monographs—the mythology of an individual race is seen to form a coherent part of mythology."

With this plan before him, Professor Alexander in Volume X. has not presented a collection of mythic stories drawn from a continent of varied aspects and conditions, but has aimed to show, as far as present knowledge will permit, the contribution that North America can offer to a world study of mythology. In the preface, he says of his subject: "The literature, already very great, is being augmented at a rate hitherto unequaled, and it is needless to say that this fact alone renders any general analysis at present provisional. As far as possible the author has endeavored to confine himself to a descriptive study and to base this study upon regional divisions."

The territory and the peoples of America north of Mexico he divides into seven regions: (1) The Far North, (2) The Forest Tribes, (3) The Gulf Region, (4) The Great Plains, (5) Mountain and Desert, (6) The Pueblo

Dwellers, (7) The Pacific Coast. A general scheme is followed in the treatment of these seven divisions. The tribes dwelling within a division are named; the environment indicated; cosmogony outlined; the deified powers and mythic characters mentioned; and the beliefs, legends, stories, briefly set forth. By such a broad sketch of each, the seven divisions are presented in the eleven chapters of the book.

Professor Alexander in his "Introduction," remarks (p. xv):

"Mythology in the classical acceptation can scarcely be said to exist in North America; but in quite another sense—a belief in more or less personified nature-powers and the possession of stories narrating the deeds and adventures of these persons—the Indians own, not one but many mythologies; for every tribe and often within the tribe, each clan and society, has its individual mythic lore." This statement he qualifies and adds the following discriminating observation. "Beliefs vary from tribe to tribe, even from clan to clan, yet there are fundamental similarities and uniformities that afford a basis for a kind of critical reconstruction of a North American mythology. No single tribe and no group of tribes has completely expressed this mythology—much less has any realized its form; but the student of Indian lore can scarcely fail to become conscious of a coherent system of myths, of which the Indians themselves might have become aware in the course of time, if the intervention of Old-Word ideas had not confused them." On p. xvi the author wisely says: "In America, no more than in the Old World, are we to identify religion with mythology. The two are intimately related; every mythology is in some degree an effort to define a religion." Attention is called to the fact that "the powers which evoke the Indian's deepest veneration are of rare appearance in the tales," and adds: "The Indian's religion must be studied in his rites rather than in his myths." On p. xviii we read: "Inevitably these powers (of nature) find a fluctuating representation in the varying imagery of myth. Consistency is not demanded, for the Indian's mode of thought is too deeply symbolic for him to regard his own stories as literal; they are neither allegory nor history; they are myth with a truth midway between that of allegory and that of history. . . . The vast majority (of Indian stories) are obviously told for entertainment; they represent an art, the art of fiction; and they fall into the classes of fiction, satire and humor, romance, adventure. Again, not a few are moral allegories, or they are fables with obvious lessons. . . . Myths that detail causes are science in infancy and they are perhaps the only stories that may properly be called myths."

Space forbids further quotation of the many discerning observations or deductions scattered throughout the pages.

One who knows something of the vast jumble of material that in this volume has been whipped into shape, can best appreciate the difficulty of the task essayed by the author and it is a pleasure to call attention to the breadth of culture and sympathy he has brought to its accomplishment. "The time will certainly come for a closely analytical comparative study of North American myths" he declares; and when that time arrives, may the task fall into equally competent hands, as the present volume.

This interesting and valuable book was not prepared for specialists, although it will be of service to such. To the general student of American history it presents a new and comprehensive view of ancient life and thought upon this continent.

Alice C. Fletcher

NOTES ON CANADIAN STRATIGRAPHY AND PALEONTOLOGY

CORDILLERAN PROVINCE

The Rossland, British Columbia, mining camp is situated in the Columbia Range immediately north of the international boundary and west of the Columbia River. A recently published memoir by C. W. Drysdale,[1] al-

[1] "Geology and Ore Deposits of Rossland, B. C.," C. W. Drysdale, Geological Survey, Canada, Memoir 77, 1915.

though devoted in the main to a description of the ore deposits, gives much valuable information concerning the stratigraphy and geological history of the region. The oldest rocks are the slates, shales, quartzites, calcareous sediments, and tuffs of the Mt. Roberts formation, aggregating over 1,200 feet in thickness. A meager collection of fossils, collected by R. W. Brock and identified by H. M. Ami, indicates the Pennsylvanian age of the formation, but gives no clue to faunal relationships. The sediments are cut by intrusive and extrusive volcanic rocks of Triassic and Jurassic age; the whole region suffered orogenic uplift at the close of the last-named period. By the end of Cretaceous times it had been peneplained and was again deformed during the Laramide revolution. Stream gravels, probably of Eocene age, are known in two localities, but the major record of mid-Tertiary time is one of volcanic activity. By the close of the Pliocene period, a late mature topography of comparatively slight relief had been carved beneath the Cretaceous paleoplain and at that time the streams were rejuvenated by another epirogenic uplift. The greater part of the present relief is the result of Quaternary stream erosion aided by glaciation.

The agricultural development of the Prairie Provinces of Canada must inevitably bring an increasing demand for phosphates, although at the present time no deposits of mineral phosphate are being worked in the Dominion. In the hope that phosphate beds similar to those of the western United States might be discovered in Alberta and British Columbia, the Canadian Conservation Commission delegated F. D. Adams and W. J. Dick to make a reconnaissance of favorable localities during the 1915 field season. The report[2] of their work was published late in 1915—an enviable record for prompt publication by a government scientific bureau. Three lines of section across the Rocky Mountains were selected as possibly exposing strata similar to the phosphate-bearing Pennsylvanian terranes of Idaho and Montana. Relying solely upon paleontological evidence, it was speedily ascertained that two of these contained no rocks of Upper Carboniferous age and attention was centered upon the third area, the Rocky Mountains Park at Banff. By faunal analogy with the Montana field, 350 miles to the south, phosphate beds might be expected to occur near the contact of the Upper Banff limestone and the Rocky Mountain quartzite. Search was rewarded by the discovery of low-grade phosphate rock in place and one piece of high-grade "float," enough to demonstrate that careful prospecting in these horizons is justifiable. The report is concluded with a number of valuable suggestions for prospectors and a summary of the phosphate resources of the world.

Recent field work carried on in the Canadian Rockies by L. D. Burling[3] has resulted in important additions to our knowledge of Paleozoic stratigraphy in that region. The ammonite-bearing shales near Massive, west of Banff, Alberta, originally described as of Jurassic age[4] are now known to represent the Upper Banff shales, are probably of Permian age, and occupy the normal position above the Rocky Mountain quartzite. Devonian and Cambrian strata are in juxtaposition over an area of 5,000 square miles between Banff and Elko, British Columbia, although only a few miles to the northwest 10,000 feet of Ordovician and Silurian strata overly the Cambrian. The Albertella fauna is of especial interest because it is the oldest Cambrian fauna found in contact with the Beltian rocks of Montana and adjacent regions. It is now known to be of early Middle Cambrian age from its discovery in strata in Mount Bosworth, British Columbia, and elsewhere. The line between the Middle and Upper Cambrian was found over wide areas to be the locus of a pronounced emergence of the sea bottom,

[2] "Discovery of Phosphate of Lime in the Rocky Mountains," F. D. Adams and W. J. Dick, Commission of Conservation, Canada, Ottawa, 1915.

[3] "Notes on the Stratigraphy of the Rocky Mountains, Alberta and British Columbia," L. D. Burling, Geological Survey, Canada, Summary Report for 1915, 1916, pp. 97–100.

[4] Geological Survey, Canada, Guide Book 8, Pt. 2, 1913, p. 191.

while the Lower and Middle Cambrian are separated by a diastrophic break of considerable magnitude.

ORDOVICIAN FORMATIONS AND FAUNAS

Nearly half of the iron smelted in Canada is obtained from the Wabana iron ore deposits on Bell Island in Conception Bay, Newfoundland. Dr. A. O. Hayes, in a recently published memoir,[5] has given an excellent description of these ores and their occurrence. Oolitic iron ore with ferrugineous shales and sandstones forms part of a sedimentary series containing a fauna which correlates with the Arenig and lower Llandeilo stages of Wales, corresponding roughly to the Beekmantown, Chazy, and Black River of the Appalachian province. The spherules of ore are composed of alternating concentric layers of hematite and chamosite (a green iron silicate) and in many cases were pierced by living boring algæ. Algæ are found in all horizons in the ore beds and doubtless played an important part in the precipitation of these primary bedded ores. Practically all of the calcium and phosphorus of the ores is derived from linguloid brachiopod shells. Layers of oolitic pyrite associated with a graptolite fauna occur in the midst of the shales between two of the iron ore zones. These are interpreted as indicative of open ocean currents and deeper water. The chapters treating of the origin of these beds make use of many data obtained from recent studies of Drew, Doss, and others, concerning the chemical reactions induced by marine bacteria.

Epicontinental seas of Ordovician time were much more basin-like in character than was formerly supposed. Difficulties have frequently arisen from the fact that minor formation names were carried over wide expanses of territory without due examination of fossil faunas. Especially is this true of the strata deposited during the latter part of the period. Faunal studies by A. F. Foerste, now in progress, are yielding very important results concerning the so-called Lorraine and Richmond terranes of Ontario and Quebec.[6] The investigations embrace two general areas: that extending from the northern shore of Lake Ontario northwestward across Georgian Bay, and that east of the Frontenac axis in eastern Ontario and southern Quebec.

The faunas of the "Lorraine" formations in the more westerly of these two basins are so different from the typical Lorraine fauna of New York that the use of the term Lorraine can be of little value. The terms Maysville and Eden may prove much more appropriate, as these strata can probably be correlated with the formations so named in the vicinity of Cincinnati, Ohio, at least in a general way. Apparently the "Lorraine" of Ontario presents much more in common with the strata of a similar age in the Ohio basin than with the Lorraine of the province of Quebec. The latter is, also, faunally distinct from the New York Lorraine and evidently represents sedimentation in a somewhat isolated basin. Quite probably the Frontenac axis was sufficiently developed in later Ordovician time to form a faunal barrier along the southern and western border of the region in which accumulation of the Quebec Lorraine was being effected.

In neither of the Canadian provinces is there a definite line of demarcation between the "Lorraine" and Richmond. The Richmond fauna seems to have invaded the "Lorraine" seas gradually, a few species at a time, rather than *en masse*. The upper part of the so-called Lorraine of Ontario is doubtless of Richmond age. The Richmond includes also the Queenston shales, largely of a red color, which occur in eastern Ontario and Quebec as well as in the vicinity of Lake Ontario. These shales appear to be merely the estuarine representatives, along the southern margin of the Laurentian highlands, of marine strata elsewhere known as the Richmond formation. The Richmond fauna of the eastern basin has a decidedly western aspect and embraces only

[5] "Wabana Iron Ore of Newfoundland," A. O. Hayes, Geological Survey, Canada, Memoir 78, 1915.

[6] "Upper Ordovician Formations in Ontario and Quebec," A. F. Foerste, Geological Survey, Canada, Memoir 83, 1916.

a small element closely akin to the Richmondian of Anticosti Island. It is probable, however, that for a brief time open waterways afforded migration by way of some northern passage from Anticosti as far west as Manitoulin Island in Georgian Bay, for a small contingent in the Richmond fauna of the western basin seems to have been recruited from the St. Lawrence gulf.

OIL AND GAS FIELDS

Brief descriptions of the Paleozoic strata of southern Ontario and Quebec are included in a treatise upon the oil and gas fields of these provinces by Wyatt Malcolm.[7] All available drill records together with statistics of production are assembled and the occurrence of oil and gas with relation to rock structure is discussed. The oil production has steadily declined in recent years, but gas production has been rapidly increasing and the fields have been widely extended. Oil or gas, or both, have been found in the Onondaga, Salina, Guelph, Clinton, and Medina formations. The prospects for new fields are not very encouraging.

Similar summary descriptions of Paleozoic strata and the logs of wells drilled for oil and gas in Ontario are assembled by C. W. Knight[8] in the current report of the Ontario Bureau of Mines.

DEVONIAN FORMATIONS AND FAUNAS

In general, the introduction of a new fauna or faunal facies is of more importance in delineating stratigraphic boundaries than is the persistence of an old biota. Applying this principle to the basal Devonian strata in Ontario, it becomes necessary to place the Detroit River series in the Devonian rather than in the Silurian system. The evidence for this conclusion is presented by C. R. Stauffer in a paper[9] which may be considered as a postscript to the same author's memoir on the Devonian of Ontario.[10] The Detroit River, or Upper Monroe, series comprises four formations: the Lucas and Amherstburg dolomites, the Anderdon limestone, and the Flat Rock dolomite, named in descending order. The Amherstburg fauna is typically Devonian with strong Onondaga affinities, but the Lucas dolomite contains a large proportion of residual Silurian forms, many of which display little or no recognizable variation from their pre-Devonian ancestors. The erosion interval between the deposition of the Detroit River series and the overlying Onondaga limestone was a long one, so that the former is probably to be referred to the Helderbergian. The faunas are, however, so distinctive that they must have existed in an embayment, presumably from the north or northwest, which was altogether isolated from that of New York and adjacent states toward the east and south.

The Gaspé peninsula in Quebec rivals Anticosti Island in the significance of its record of mid-Paleozoic times. Not the least interesting of its sections is that of the escarpment of the Table-À-Rolante at Percé, which extends northwestward and southeastward from the Pic d'Aurore. John M. Clarke's description[11] of this exposure is artistically illustrated by a colored reproduction of the brilliant cliffs which overhang the Mal-Baie. The summit of the cliffs consists of horizontal strata of Bonaventure conglomerate, a typical "Old Red" sandstone of later Devonian, and possibly in part Mississippian, age. Unconformably underlying that formation are the Percé limestone and the Pic d'Aurore series. In the midst of the latter is a sandstone band carrying the typical sand fauna of the New York Oriskany. Faulted up at the east is a block of Barré limestone, of earliest Devonian age. Beneath the Devonian strata are highly contorted Silurian and Ordovician sediments. The shoal water Oriskany sands form a striking contrast to the lower portion

[7] "The Oil and Gas Fields of Ontario and Quebec," Wyatt Malcolm, Geological Survey, Canada, Memoir 81, 1915.

[8] "Oil and Gas in Ontario," C. W. Knight, Ontario Bureau Mines, 24th Ann. Rept., Pt. 2, 1915.

[9] *Bull. Geol. Soc. America*, Vol. 27, 1916, pp. 72–77.

[10] Geol. Surv., Canada, Memoir 34, 1915.

[11] N. Y. State Museum, Bull. 177, 1915, pp. 147–153.

of the Grande Grève limestone, which outcrops elsewhere on the peninsula and carries a similar Oriskany fauna of overgrown sand-loving invertebrates imprisoned in a calcareous matrix. As in New York, both the shallow and deep water facies of the Oriskany are present at Gaspé, but without the striking differences in faunal content. The Bonaventure conglomerate, in its lithology as well as in its structural relations to the underlying formations, bears testimony to the importance of the mid-Devonian orogeny in the northeastern Appalachian province. Dr. Clarke in another paper[12] in the same publication lays stress upon this diastrophy and couples with it the volcanic activity responsible for the Monteregian hills.

TRIASSIC FORMATIONS

Detailed descriptions of the Newark series, as exposed along the shores of Minas Basin and the Bay of Fundy, are given by Sidney Powers in an important contribution concerning the Acadian Triassic.[13] The area is the most northerly of the geo-synclinal basins developed in the Atlantic coastal province during the Triassic period and presents problems similar to those of the Connecticut Valley. Sedimentation was largely fluviatile, in the main resulting from the occasional floods of a hot dry climate. Fissure eruptions and volcanic ejections occurred at intervals during the accumulation of the sediments.

PLEISTOCENE (?) MAN IN BRITISH COLUMBIA

Fragments of a human skeleton were discovered in 1911 near Savona, B. C., in silt beds alleged to be of Pleistocene age. A claim of great antiquity for the skeleton was made before the Royal Society of Canada in May, 1915. The bones are those of an aged woman and display no characters that would distinguish them from those of a modern Shuswap Indian.[14] An investigation of the deposits from which the bones were obtained was undertaken by C. W. Drysdale. He reports[15] that the field evidence indicates the Recent age of the silts at the locality. There is, therefore, no basis for the belief that the Savona skeleton is a relic of Pleistocene man.

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KINGSTON, CANADA,
September 7, 1916

[12] N. Y. State Museum, Bull. 177, 1915, pp. 115–134.

[13] *Jour. Geol.*, Vol. 24, Nos. 1, 2 and 3, 1916.

[14] Knowles, F. H. S., Geol. Surv., Canada, Summary Report for 1915, 1916, pp. 281–283.

[15] "Human Skeleton from Silt Bed near Savona, B. C.," C. W. Drysdale, Geological Survey, Canada, Summary Report for 1915, 1916, pp. 91–92.

METHODS OF CRITICISM OF "SOIL BACTERIA AND PHOSPHATES"[1]

A CIRCULAR letter, dated July 28, 1916, criticizing Bulletin 190 ("Soil Bacteria and Phosphates") of the University of Illinois Agricultural Experiment Station was sent to many editors of agricultural journals. This letter bears the signature of Dr. H. J. Wheeler, of the Agricultural Service Bureau of the American Agricultural Chemical Company.

The caption employed is as follows: "Confidential and Not For Publication." This will doubtless appear to those who welcome frank and open criticism as an entirely unwarranted and a highly undignified manner of criticism. No copy of this letter was received by us from Dr. Wheeler, but through the courtesy of the agricultural press the matter has reached us from many sources and from several different states.

The purpose of Dr. Wheeler's letter to the agricultural editors is evidently to belittle the importance of the discovery that the nitrifying bacteria have power to make rock phosphate soluble.

As space will not permit quoting in full the contents of this four-page letter, we quote only the statements under discussion. In the last paragraph of the first page, we read:

> The organic acids and the carbonic acid produced in the decomposition of vegetable matter or brought

[1] SCIENCE, p. 246, August 18, 1916, and Bulletin 190, University of Illinois Agricultural Experiment Station.

down in the rainfall, including also nitrous and nitric acid, produced as described above, tend to unite in the soil with the most readily attackable bases in the basic silicates and with the lime of the carbonate of lime before they can attack raw rock phosphate effectively.

That the acids attack the basic silicates is agreed, and this means that some of the potash minerals naturally contained in the soil are thus rendered soluble, and the potassium contained in them made available to higher plants. Potash minerals are important and abundant constituents of all normal soils. The idea conveyed by this part of the quoted statement is in agreement with the published results of the Illinois Experiment Station[2] and was sufficiently considered by the authors of Bulletin 190, as the following quotation indicates:

The nitrous acid produced may act upon compounds of iron, aluminum, potassium, sodium or magnesium which occur in soils or it may act upon tricalcium phosphate, calcium silicate or calcium carbonate, if present.

To intimate, as Wheeler does, that the acids will choose to unite first with these basic silicates before attacking effectively the raw rock phosphate, is not in accord with the scientific facts, nor does it appear to be sound chemical reasoning, and no mention is made by Wheeler of the acid silicates contained in the soil which may react with raw rock phosphate. To establish minute acid and alkaline areas in the soil is to approach the ideal for both the biological and chemical factors to work in unison in liberating insoluble materials. Ground limestone made up of various degrees of fineness actually provides alkaline areas, and fermentations of organic residues are always developing acid areas. As raw rock phosphate is dissolved by acids, whether produced by bacteria or by chemical means, and, as it is so placed that some of it must have contact with the fermenting areas, it is not difficult to understand why it becomes soluble and why it produces increased crop yields, even though at many other points in the soil centers of alkalinity exist.

The tricalcium phosphate used by us is commented upon by Dr. Wheeler as "an especially soluble artificial tricalcium phosphate." If tricalcium phosphate was a soluble material, which of course it is not, there would have been no logical reason for conducting the experiments.

The "high temperature" of the experiment is mentioned by Wheeler, and, although of minor importance, we note from the records that during the seventy-eight days from July 4 to September 18, 1916, there were sixty-one days on which the temperature of the air ranged above that of the experimental laboratory. At best, temperature could only influence the rate of biochemical action in this experiment.

In regard to the statements bearing on the extent of this solvent action of nitrite bacteria in soils, it is necessary to quote in part the last paragraph of Wheeler's letter.

In their hope of confining the solvent action of the nitrous acid as fully as possible to the raw rock phosphate, Hopkins has recommended that the phosphate be turned under in intimate contact with organic matter, yet when one realizes the even closer contact of the many soil particles with the organic matter at the same time, it will be obviously impossible for the nitrous acid to attack wholly or even chiefly the raw rock phosphate.

In the table below taken from Illinois Bulletin 190 are presented the amounts of phosphorus, calcium, and nitrogen required by standard crops, and the amounts of phosphorus and calcium which would be made soluble if all the nitrogen required for the crop should be oxidized to nitrate and should act upon pure rock phosphate.

The figures show that there is possible of solution from this biochemical process about 7 times as much phosphorus as corn, wheat, or oats require, and 9 times as much as timothy requires. Greater differences occur in the calcium figures, there being possible of solution 14 times that required for corn, 18 times that required for wheat, 12 times that required for oats, and 8 times that required for timothy.

Nowhere in Dr. Wheeler's letter has he pointed out that the nitrous acid furnishes

[2] Bulletin 182, "Potassium from the Soil."

from seven to nine times the necessary solvent power.

PHOSPHORUS, CALCIUM AND NITROGEN REQUIRED BY CROPS, COMPARED WITH THAT POSSIBLE OF SOLUTION WHEN NITRITE BACTERIA ACT UPON TRICALCIUM PHOSPHATE

(Expressed in pounds)

Crop	Nitrogen	Phosphorus		Calcium	
	Required	Required	Possible.	Required	Possible
Corn: Grain, 100 bu.. Stover, 3 tons. Cobs, ½ ton...	150	23	166	22	321
Wheat: Grain, 50 bu... Straw, 2½ tons.	96	16	107	11	206
Oats: Grain, 100 bu.. Straw, 2½ tons.	97	16	108	17	208
Timothy, 3 tons.	76	9	84	20	163

The authors desire to point out that the figures in the table were based upon the solvent action which only the nitrogen would exert when it is oxidized. The associated acid radicle as stated on page 402 of the bulletin may make equal amounts of phosphorus and calcium soluble, thus doubling that reported above. Thus, corn, wheat and oats only require one fourteenth of the phosphorus possible of solution when the acid formed by the nitrite bacteria and that which was combined with the oxidizable ammonia both act upon the raw rock phosphate. It may be that the phosphorus would be made available even if only converted to the dicalcium form, which would require only half as much acid.

The recent results of the New Jersey Station on the oxidation of sulfur and its solvent action on raw rock phosphate in soils support the statement that phosphorus is made soluble by biochemical oxidation in large amounts even in the near presence of other bases.[3]

Limited space is taken to include some field data from the large mass available, showing the advantage of rational systems of permanent soil enrichment in crop rotation, in which raw rock phosphate, limestone and "home-grown" organic matter are the only materials used.

WHEAT YIELDS PER ACRE: FOUR-YEAR AVERAGE UNIVERSITY OF ILLINOIS FARM

Soil Treatment	Bushels
Crop residues	27.6
Manure	28.8
Crop residues, phosphorus	42.0
Manure, phosphorus	45.6
Crop residues, phosphorus, limestone	46.8
Manure, phosphorus, limestone	48.9

These figures demonstrate sufficiently the advantage of mixing organic materials and raw rock phosphate as judged by increased crop yields, and they show that limestone, even when used with raw rock phosphate under field conditions, gives a further increase over that produced by the combination of organic materials and raw rock phosphate.

In his letter, Dr. Wheeler quotes from Director Thorne of the Ohio Agricultural Experiment Station as follows:

> Where we have used floats (raw rock phosphate) as a reenforcement of manure on this farm alongside of acid phosphate, the acid phosphate has given us a slightly larger net gain in the average of the 18 years' work, and a decidedly large gain during the last half of the period.

But Wheeler fails to point out that this statement is based upon only one of the two methods used by Director Thorne in computing the increase produced by the phosphates. He might have quoted from Director Thorne's writing as follows:[4]

> On Section *C*, Plots 1 and 11, which, it will be observed, are continuous, have regularly given yields so much larger than those of the other unmanured plots of this section as to suggest the possibility that the land covered by these plots may have been at one time occupied by a fence-row, the tract lying near a barn, and for this reason it has been deemed best to calculate the increase on the general average of all the unfertilized plots. By this method of calculation the average increase on Plots 2 and 3 combined (with raw phosphate) is found to be practically the same as on Plots 5 and 6 combined (with acid phosphate) but when the larger cost of the acid phosphate is deducted the net gain is a little greater on Plot 2 and 3.

[3] *Soil Science*, p. 533, June, 1916.

[4] P. 18, Ohio Experiment Station Circular 104.

On page 175 of Director Thorne's excellent book on "Farm Manures," he also shows that, when the increase is computed by the method which he states "has been deemed best," the net profit is greater per acre, and very much greater per dollar invested, from raw rock phosphate than from acid phosphate.

ACRE-YIELDS IN OHIO MANURE-PHOSPHATE EXPERIMENTS: AVERAGE OF 9 YEARS, 1906–1914

Phosphate	None	None	Rock	Rock	Acid	Acid
Manure	Yard	Stall	Yard	Stall	Yard	Stall
Corn, bushels	54.4	62.7	68.3	72.1	65.8	68.9
Wheat, bushels	24.3	25.5	27.2	28.2	29.8	29.4
Hay, tons	2.00	2.34	2.43	2.60	2.43	2.68
Value 3 crops	$57.20	$65.20	$68.52	$72.20	$69.60	$72.52
Phosphate gains	......	......	11.32	7.00	12.40	7.32
Phosphate cost	......	......	1.20	1.20	2.40	2.40
Phosphate profit	......	......	10.12	5.80	10.00	4.92
Profit per $1	......	......	8.43	4.83	4.17	2.05

The accompanying table gives the average of the actual yields secured in these Ohio experiments during the last half of the eighteen-year period.

If we value the corn at 40 cents a bushel, the wheat at 80 cents, and the hay at $8 a ton, and count the cost of rock phosphate at $7.50 per ton and acid phosphate at $15, we find, by this direct method of computation, that the rock phosphate was slightly more profitable per acre, and more than twice as profitable per dollar invested, as the acid phosphate.

In his letter to the agricultural editors, Dr. Wheeler quotes a personal letter from Professor Mooers expressing his opinion as to the conclusions which should be drawn from experimental data, in part unpublished, secured by the Tennessee Experiment Station. If this opinion is based upon a continuation of the experiments in which two crops (wheat followed by cowpeas) were grown every year on the same land, as reported in Tennessee Experiment Station Bulletin No. 90, in which on page 89 it is shown that for each dollar invested rock phosphate paid back $2.29, and steamed bone meal only $1.90, and in which the use of steamed bone meal is commended and the use of rock phosphate discouraged, we must await further publication by the Tennessee Station before accepting Wheeler's confidential report of Mooers' personal opinion.

For more complete data from the phosphate experiments conducted by many state experiment stations, the interested reader is referred to Illinois Experiment Station Circular 186, "Phosphates and Honesty."

Cyril G. Hopkins,
Albert L. Whiting
University of Illinois

SPECIAL ARTICLES

THE LIGHT-PRODUCING SUBSTANCES, PHOTOGENIN AND PHOTOPHELEIN, OF LUMINOUS ANIMALS

In a previous issue of Science (N. S., XLIV., 208, 1916), I called attention to Dubois's discovery of substances called luciferin and luciferase in the West Indian "cucullo" *Pyrophorus noctilucans,* and the mollusc, *Pholas dactylus.* I also recorded the existence of similar bodies in the fire-flies, *Photinus* and *Photuris,* and of luciferin in luminous bacteria. Luciferase, according to Dubois, a thermolabile enzyme capable of accelerating the oxidation of luciferin, is prepared by allowing an extract of luminous cells to stand until the light disappears. The luciferin is thus completely oxidized and used up. The luciferin, according to Dubois, a thermostabile substance capable of oxidation with light production, is prepared by extracting the luminous cells with hot water which destroys the luciferase but not the luciferin. Light will appear if we mix the solutions of luciferin and luciferase in presence of oxygen. Each

substance alone in solution is non-luminous and fairly stable. On this theory, therefore, the luciferin is the source of the light and, according to Dubois, *Pholas* luciferin will give light on oxidation with $KMnO_4$, blood, H_2O_2 and similar oxidizing agents. He found also substances (luciferase) in the blood of various marine molluscs and crustaceans which would give light with *Pholas* luciferin, but the latter he found only in the luminous organs of *Pholas dactylus*.[1]

Since the publication of my previous paper, I have investigated very thoroughly the chemistry of light production in five different forms:[2] the Japanese fire-flies, *Luciola parva* and *L. vitticollis;* an ostracod crustacean, *Cypridina hilgendorfii;* a squid, *Watasenia scintillans;* a pennatulid, *Cavernularia haberi;* and the protozoan, *Noctiluca miliaris. Watasenia, Cavernularia* and *Noctiluca* will not give the luciferin-luciferase reaction despite the most favorable conditions and many attempts to demonstrate it. These organisms need not be considered at present, as there are many reasons why the luciferin-luciferase reaction might fail.

Cypridina and *Luciola* both contain bodies similar to luciferin and luciferase, but in these forms the production of light differs in very essential points from that described by Dubois in *Pholas,* and I have come to quite different conclusions regarding the nature of the substances concerned. We may conveniently use Dubois's terminology for the present. First, in *Cypridina* and *Luciola,* it is the luciferase which is found only in the luminous cells, and luciferin is widely distributed in non-luminous forms. Second, I have been unable to oxidize luciferin with light production by $KMnO_4$ or other oxidizing agents. Third and most important, *Cypridina luciferase* will give light with substances (NaCl crystals, thymol, butyl alcohol, saponin), *some of which could not possibly be oxidized.* The *luciferase* and not the luciferin is therefore the *source* of the light. Instead of luciferin oxidizing with light production through the catalytic action of luciferase, luciferin is a body assisting in the evolution of light from luciferase. I therefore propose the new names of *photogenin* (light producer from *phos,* light, and *gennao,* to produce) for luciferase, and *photophelein* (light assister from *phos,* light, and *opheleo,* to assist) for luciferin, to indicate more clearly the nature of the light-producing process. *Cypridina* photophelein (= luciferin) in addition to its thermostabile property is easily dialyzable, while photogenin (= luciferase) is not. In these points and some others, the system resembles and may be compared to the zymase system (enzyme and coenzyme) of yeast cells.[3] As in so many other biological reactions an easily diffusible thermostabile substance (coenzyme) and a difficultly diffusible thermolabile substance (zymase) are concerned.

The light-producing power of photogenin and photophelein is very extraordinary. *Cypridina* photogenin will give visible light with photophelein in one part to 1,600,000,000 parts water. Even this is an underestimate, as we do not know the concentration of photogenin in the luminous cells apart from proteins, water, etc. In the small amounts necessary to produce light and in destruction by boiling, photogenin resembles an enzyme but differs in the fact that it is used up in the reaction. Experiment has shown that it takes photophelein from one hundred *Cypridinas* to use up the photogenin from one *Cypridina.* Perhaps the fact that photogenin is used up is not sufficient evidence to condemn it as an enzyme since many enzymes are poisoned or destroyed by reaction products; nevertheless I have deemed it best for the present to avoid the termination *ase.*

Cypridina and the firefly differ from *Pholas* in the points enumerated above and agree in most properties with each other, but with some exceptions. For instance, firefly photogenin is readily destroyed by chloroform or

[1] Dubois, R., *Annales de l. Soc. Linn. de Lyon,* 1913 and 1914.

[2] Studies made in Japan under the auspices of the department of marine biology, Carnegie Institution of Washington.

[3] Harden, A., and Young, W. J., *Proc. Roy. Soc., B,* 77, 405, 1906, and 78, 369, 1906.

ether and is a very unstable substance. Firefly photophelein is not harmed by chloroform and can be preserved for many days. On the other hand, it is the *Cypridina* photophelein which is the unstable substance. A water solution of *Cypridina* photogenin preserved with chloroform for 56 days will still give light on mixing with fresh photophelein. It should be borne in mind that photogenin, the source of the light, is not only a very powerful substance, but also a stable substance. If we can see the light from a stable body in a concentration of 1:1,600,000,000, what might not be accomplished with the pure substance? We have, perhaps, in the power of photogenin the first indication of a really possible utility of "cold light." My work is not sufficiently advanced to state the chemical nature of photogenin except to say that it is probably protein. Many of the properties of photogenin and photophelein will be found in forthcoming papers on *Cypridina, Cavernularia* and the firefly.

The photogenin and photophelein of *Cypridina* are secreted together into the sea water as a perfectly clear granule-free secretion from gland cells on the upper lip, but as already mentioned, in the body, photophelein is found throughout the animal, probably in the blood, photogenin only in the luminous cells. Just as in presence of photogenin, photophelein is rapidly used up with light production, so in presence of extract of the non-luminous cells of *Cypridina,* photophelein quickly disappears, but without light production. If we boil the non-luminous cell extract or exclude oxygen, the photophelein is not so rapidly used up. In the case of the firefly, the photophelein disappears so rapidly from an extract of non-luminous cells that it is necessary to extract them with boiling water to prepare a stable solution giving light with photogenin. Because of failure to boil the extract, I previously had overlooked the existence of photophelein in the non-luminous parts of fireflies. The evidence seems to indicate that boiling destroys a substance existing in non-luminous parts which oxidizes the photophelein.

Probably photogenin from different forms is different, at least there is a certain amount of specificity in the photogenin-photophelein reaction. Photogenin from *Cypridina* will give a faint light with photophelein from the firefly, but photogenin and photophelein of the same species or allied species give much the brightest light. For instance, firefly photogenin will give a brighter light with photophelein from other species of fireflies or even from *non-luminous* insects (*i. e.,* the boiled cell extracts of non-luminous beetles) than with *Cypridina* photophelein. Indeed, it may be found that the photogenins from different forms exhibit differences in light-giving power, depending on relationship, similar to the differences in the hemoglobins, or similar to the specificity of the precipitin reactions of different animals.

If Dubois's statement that *Pholas* luciferin will give light with oxidizing agents, that it is not destroyed by heat and is found only in luminous cells, be confirmed, we may perhaps look to two general methods of light production in the animal kingdom—one as in *Pholas,* the oxidation with light production of *luciferin* by luciferase so closely paralleled by pyrogallol and peroxidases;[4] the other, as in *Cypridina* and the firefly, through the interaction of photogenin and photophelein, the *photogenin* giving light by some mechanism which can not at present be definitely stated. The closest parallel is the zymase system. Just as zymase is inactive without its co-enzyme, so photogenin is inactive (will not emit light) without photophelein, and just as there are certain quantitative relations between zymase and co-zymase, so there are similar quantitative relations between photogenin and photophelein. As oxygen is necessary for light production, we may, perhaps, provisionally regard photogenin as a substance auto-oxidizable with light production only in the presence of photophelein.

E. Newton Harvey

Princeton University,
October 16, 1916

[4] Harvey, E. N., *Amer. Jour. Physiol.,* 1916, XLI., 454.